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The Semiotics of Quantum-Non-Locality

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THE SEMIOTICS OF QUANTUM-NON-LOCALITY

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ABSTRACT

The paradox of Einstein, Podolski and Rosen was evoked to point out the incompleteness of quantum mechanics. The idea was that the predictions of quantum mechanics could not be trusted in cases where it contradicted the principle of local realism. This principle has normally been considered closely connected to theories of local hidden variables, although this connection is not drawn in the Einstein, Podolski, Rosen paper. It is argued in the present paper that recent experiments by Aspect and others, although they have confirmed quantum mechanics and disproven local hidden variables through the application of Bell's inequalities, have neither disproved the incompleteness of quantum mechanics nor the general principle of local realism. By applying the principle of synechism with the methods of semiotics invented by the american philosopher C. S. Peirce it is shown that it is possible to define "local realism" by continuity of interaction such that quantum mechanics itself is local realistic without hidden variables. As a consequence of this viewpoint it is shown that the validity of the quantum formalism in cases where it contradicts Bell's inequalities will depend on the connectedness, through coincidence counters or similar devices, of the experimental device. It is suggested that an experiment like the first of Aspect's but without these connections will lead to results in accordance with Bell's inequalities.

THE SEMIOTICS OF QUANTUM-NON-LOCALITY.

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CONTENTS

1. The completeness of quantum theory.

The philosophical debate concerning the Einstein-Podolski-Rosen (EPR) paradox [1.] has reached a new climax recently. Based on Bell's operational conception of Einstein-locality [2.] several successful experiments have vindicated quantum mechanics [3.]. Most convincingly the series of experiments performed by Aspect and coworkers have demonstrated the validity of the quantum mechanical formalism [4.].

Still, the original question posed by Einstein, Podolsky and Rosen "Can Quantum Mechanical Description of Physical Reality be Considered Complete?" remains unanswered. The question of completeness regarded experimentally can only give a clear answer by contradicting quantum mechanics, an experimental affirmation can never with certainty lead to the conclusion that quantum mechanics is complete. The belief in the completeness of the theory as expressed by Niels Bohr [5.] seems to be widespread in the physical community, to a degree such that many would deny that an alternative outcome of Aspect's experiments is thinkable and that such experiments therefore are of only slight interest.

Because completeness cannot be proven experimentally, belief in it can only be maintained by pure conviction, or on logical grounds. A logical proof of completeness for a physical theory like quantum mechanics would probably be more difficult to establish than a completeness proof for a mathematical theory, say the theory of whole numbers, for two reasons: First, the mathematical theory is a part of the physical theory, and, second, the semantical questions of interpretation of the physical symbols are much more intricate than the interpretation of mathematical symbols. A formalism cannot prove itself with formulæ alone, and questions of consistency and completeness are meaningless unless an interpretation is provided so one cannot escape from problems of semantics or semiotics. In mathematics and formal logic an interpretation is normally considered as context-free and formally treated like a mapping of the symbols of the language onto the objects in a domain under investigation [6.]. In physics, however, and especially in quantum mechanics, an interpretation is highly dependent on the context of measurement and does really not exist without this context. This important semantic thesis of quantum mechanics was stressed by Bohr in his answer to EPR [5.]:

> "There can be no question of any unambiguous interpretation of the symbols of quantum mechanics other than that embodied in the well-known rules which allow to predict the results to be obtained by a given experimental arrangement described in a totally classical way."

Although Bohr claimed to have refuted the EPR conception of incompleteness, it looks as if the above quotation indirectly points to another sign of incompleteness. If the meaning of the wave function can only be grasped on the basis of a classical description of a measuring apparatus then it is presupposed that sufficiently ideal measuring equipment can be designed and built, but it is dubious if quantum mechanics proper can give us an exact description of the demands to be met by a measuring apparatus in order for it to be "sufficiently ideal". After all, it is thinkable that an experimental physicist who is convinced of the validity of quantum mechanics could perform an experiment with a result in disagreement with the formalism. Such an experiment could not be used as a reference for defing the meaning of the wave function because, presumably, something is wrong with the apparatus, and, according to Bohr, it should be possible to define "in a totally classical way" exactly what the error is. Bohr's definition of the meaning of the wave function points to the existence of a conceptual discontinuity, a "cut" associated with the physical location of an "interface" beyond which a quantum description is meaningless, and this of course would mean that quantum mechanics is incomplete, although in another sense than that implied by the EPR argument. An orthodox Copenhagen interpretation would hesitate before the acceptance of such an incompleteness by appealing to the correspondence principle and the belief expressed by Bohr that the "cut" or "interface" could be pushed arbitrarily long inside the apparatus towards the classical degrees of freedom. The validity of this assumption, however, is rather dubious due to the difficulties encountered in the quantum description of irreversible processes which necessarily must be of prime importance in a measuring/amplifying device, so a theoretical proof of completeness along these lines is bound to run into insurmountable obstacles.

The EPR paper was published in 1935 shortly after Gödel's proof (1931) of the incompleteness of mathematical formal systems including Peano's axioms of whole numbers. Gödel had shown that neither consistency nor completeness could be proven formally, but that consistency implied incompleteness. Probably Einstein conceived a parallel situation with respect to quantum mechanics although he did not refer to Gödel's proof. After his discussions with Bohr at the Solvay-meetings 1927-30 he seems to have been convinced that quantum mechanics at least was consistent in the sense that it could describe a limited part of the physical reality without leading to contradictions. It would then seem natural to look for signs of incompleteness and this should not be regarded as a rejection of the formalism. However, in Copenhagen the shift in Einstein's attitude seems to have been unnoticed, and the EPR-paper was considered as just another new attack on quantum mechanics [7].

While the fate of incompleteness is acceptable, inconsistency, of course, would be disastrous. It is a common misunderstanding that the EPR paradox is intended to point out an inconsistency in quantum mechanics, so it is important to stress that the logic of the paper only points to incompleteness by evoking a paradox that arises as a consequence, not of the formalism per se, but from the metaphysical belief in the universal validity of the formalism even in cases where it contradicts the principle of local realism, or Einstein separability. The philosophical question remaining is therefore if the Aspect experiments can be claimed to have disproven the notion of local realism that permeates the EPR paper.

2. Local realism and connectedness.

Due to the analyses by Bell and their follow up by Clauser, Horne, Shimony, Aspect and others it seems tempting to conclude that theories based on objectively local hidden variables have been disproved, although some small loopholes exist and several authors maintain their scepticism [8.]. If local realism was synonymous with local hidden variables there would be sufficient reason to cling to theese loopholes, but there is a difference, and it is a main purpose of this paper to point out that quantum mechanics can be locally realistic without hidden variables and without strange effects associated with the loopholes. To the opinion of the author an experiment in the spirit of EPR is possible and may falsify local realism, but it has not been performed yet.

There is a strange discrepancy between the way experiments like Aspect's are described in theoretical reviews and the way it is performed in reality. In theory one measures the correlation in polarization of two photons emitted in a cascade by performing two independent polarization measurements on the individual photons. For example, Mermin in his popular description [9.] emphasizes the importance of the disconnectedness of the two individual detectors:

> "there are neither mechanical connections (e.g. pipes, rods, strings, or wires) nor electromagnetic connections (e.g. radio, radar, or light signals) nor any other known relevant connections. Irrelevant connections may be hard to avoid. For example, all three parts may sit on the same table top."

For a naive consideration it is difficult to reconcile Mermin's remark with the circuit diagrams or photos of actual experimental setups from Freedman and Clauser to Aspect and coworkers. All these pictures show clearly that the two distant sets of single-particle detectors are well connected with solid wires to some "central black boxes", like coincidence counters and/or time-to-amplitude converters. Apparently, there must exist a tacit agreement that these connections are to be considered "irrelevant" in spite of the crucial role they play in the experiments.

This common agreement can only be justified by reference to the processes of amplification in the electronic devices, which according to classical causal logic ensures that signals are propagated easily from the single-particle detectors to the central black boxes, but not the other way, at least not to a degree such that correlated signals propagating the other way could have any significant effect on the quantum expectations.

The question is: is the justification given above for the irrelevance of these connections well enough founded in a quantum mechanical context? Isn't it just in denying the possibility of a pure one-way communication that quantum mechanics distinguishes itself most significantly from classical mechanics? As Bohr put it in his EPR-reply [5.] :

> "The finite interaction between the object and measuring agencies conditioned by the very existence of the quantum of action entails - because of the impossibility of controlling the reaction of the object on the measuring instruments if these are to serve their purpose - the necessity of a final renunciation of the classical ideal of causality and a radical revision of our attitude towards the the problem of physical reality."

This was the viewpoint that proved itself successful in the Solvay discussions between Bohr and Einstein (1927-30) on the consistency of quantum mechanics. In his reply to EPR, however, Bohr mixes it with a more "idealistic" or logical argument in a rather confusing manner. After having mentioned the EPR problem whether a measurement of the state of "particle 1" can be said to immediately determine the state of "particle 2" he writes:

> "of course there is in a case like that just considered no question of a mechanical disturbance of the system under investigation during the last critical stage of the measurement procedure. But even at this stage there is essentially the question of an influence on the very conditions which define the possible types of predictions regarding the future behavior of the system."

It is not entirely clear what the connection is between the "finite interaction" in the first quotation and the "very conditions" in the second. When we make a measurement on particle 1 we have a free choice of measuring one or the other of two complementary properties of particle 1, e.g. position or momentum. The choice between the two different ways of interaction with particle 1 according to Bohr prohibits the use of the EPR-term "the same reality" for particle 2 even though this particle is not affected by any mechanical disturbance, and this is because quantum mechanics forces us to regard the whole phenomenon of preparation and measurement as possessing an "individuality completely foreign to classical physics". The "very conditions" in the second quotation thus seems to be the formalism of quantum mechanics associated with the philosophy of complementarity, and Bohr is trying to persuade the reader to accept that quantum mechanics defines how the term "physical reality" may be correctly used. This is of course difficult to accept from a realistic standpoint: when the question is whether the quantum mechanical descprition of physical reality is complete then the answer that quantum mechanics itself defines what "physical reality" is looks like a philosophical shortcircuit or cheating in the game of debate.

Apart from breaking with realism the introduction of the "very conditions" also manages to break with the locality principle in a subtle way. When we measure one or the other of the two complementary properties of particle 1 without disturbing particle 2 then it is true that quantum mechanics gives an unambiguous prediction for the future behavior of particle 2, viz. a wave function corresponding to a pure state of that particle. The "finite interaction" is in this case only involving particle 1, but it produces via the "very conditions" a change of state of particle 2. Bohr seems to forget that we still have the possibility of making an independent measurement on particle 2 and that this would amount to a test of the formalism that can be performed whether one accepts the influence via the "very conditions" or not. What about the "finite interaction" with particle 2 that would be introduced by such a second measurement? Can we be sure that it doesn't produce a conflict with the "very conditions" if there are no physical connections between the two measurements? These questions are unanswered in Bohr's article and according to the orthodox Copenhagen interpretation of quantum mechanics one is really not allowed to ask such questions. Now that experiments have been performed of which the majority confirm quantum mechanics there is a danger that such a prohibition will be enforced to prevent a closer theoretical study of the measurement situations and the semantics of quantum theory.

It is tempting to compare Bohr's two conflicting (?) points of view, the physical and the logical, from the standpoint of the so called synechistic philosophy created by the great american thinker C. S. Peirce (1839 - 1914) [10]. The synechism of Peirce is based on semiotics, the logic of signs and relations, and on the belief that our symbolic concepts in the physics are connected with other signs in a psycho-physical continuum [10c]. Ideas interact with matter by close contact, says Peirce, and in particular, the process of measurement establishes the point of contact between the symbolic signs of physics and the indexical signs of nature. Undoubtedly Peirce, if he had had the possibility of studying the Einstein - Bohr discussion, would have supported Bohr's physical viewpoint (as well as his thesis that God plays dice), but he would probably not support Bohr's logical viewpoint in the last quotation, because it tries to circumvent a synechistic explanation.

There is also another point in the last Bohr-qoutation that disagrees with Peirce's general philosophy. The "very conditions" Bohr speaks about are formulated by quantum mechanics, so the argument bites itself in the tail and tries to persuade the reader to believe that quantum mechanics is complete, almost by definition. According to Peirce (and later, Karl Popper) the best criterion for the genuine scientific status of a theory is its "fallibility" i.e. its ability to put its ideas to a crucial test that conceivably might falsify the theory.

The synechistic philosophy of Peirce is very general definition of local realism. By combining it with relativistic ideas of the absence of "action at a distance" and the finite limiting velocity of signal propagating it is further sharpened to encompass the notion of Einstein separability. There is no conflict between synechism and modern quantum field theory because the latter is built upon the synechistic principle of local interactions. The integrity of Peirce's philosophy is exhibited by the observation that EPR experiments can be regarded as falsification tests of synechism. If the Aspect experiments can be said to have falsified synechism it is of course on a very isolated point having to do with the ill understood process of quantum collapse but with no direct consequence for the wave equations, but still, the consequences of such a falsification would be serious because it would undermine the locality principle of quantum field theory and open up for wild speculations apparently with no connections to the main body of physics.

However, the lack of connectedness in the ideal EPR experiment and the strong connectedness in the real experiments pointed out above leads to the idea that a synechism-falsifying experiment has not yet been performed. A synechistic explanation of the quantum mechanical correlations observed in the Aspect experiments based on the connectedness of the experimental equipment would be in the spirit of Peirce's (and also in the earlier spirit of Bohr's). Also, it would indicate directly how the crucial experiment could be performed, e.g. by literal implementation of Mermin's remark on the absence of connections.

3. Causal logic of the quantum collapse.

The abrupt changes described as quantum jumps has since Bohr's early theory of the hydrogen atom been a major obstacle to the visualization of quantum mechanical concepts, alien as it is to our intuitive notion of continuity expressed in Leibniz' thesis Natura non facit saltus. Bohr was painfully aware of the difficulty but insisted that it has no meaning to think of an electron in a state during the jump in between stationary states. Later developments softened this view but the irreducible quantum jump survived in the process described as the collapse or the reduction of the wave function in connection with measurements.

A philosophical "explanation" of the collapse as a transition from potentiality to reality is easy to formulate but difficult to be satisfied with in the long run, because of the lack of clarity of these two philosophical terms. The striking success of using the concept of a wave function to explain phenomena like superfluidity and other macroscopic effects has shown the wave function as much more real than a mere potentiality or a way of expressing our predictions for hypothetical measurements. The problem is that mutually exclusive or complementary "potentialities" have ways of expressing themselves as "real" in other ways than by proper measurements (one of the main points of the EPR paper). If one can perform a measurement in one point of space and thereby specify which potential property is realized in another point without physical connections to this other point then it is difficult to avoid thinking that the potentialities are somehow real before the measurement. On the other hand it leads to contradictions if one tries to erase the distinction between wave functions and probabilities as shown by v. Neumann. It is as if the wave function in an uncanny way knows in advance which of its latent possibilities will be realized by an experiment, and this has lead to many strange ad hoc theories of "backward causality" or "splitting universes".

As stressed by Bohr the classical notion of causality must be abandoned when we discuss the quantum mechanical measurement process. The classical idea of cause-effect relationship is based on the assumption that signal variables exist, having definite numerical values independent of our measurements. This assumption was already heavily criticized by Peirce in 1892 in "The Doctrine of Necessity Examined" [10b.]. Peirce's viewpoint is that the meaning of a symbol like a number on a continuous scale, with uncertainty inherent, exists only in the context of a measurement and a statistical procedure. Therefore, we ought to drop the idea that exact numerical values of continuous quantities exist by themselves in nature as well as the idea of exact (classical) causality, and we must allow God the freedom to play dice:

"Those observations which are generally adduced in favor of mechanical causation simply prove that there is an element of regularity in nature, and have no bearing whatever upon the question of whether such regularity is exact and universal, or not. Nay, in regard to this exactitude, all observation is directly opposed to it; and the most that can be said is that a good deal of this observation can be explained away. Try to verify any law of nature, and you will find that the more precise your observations, the more certain they will be to show irregular departures from the law. We are accustomed to ascribe these, and I do not say wrongly, to errors of observation; yet we cannot usually account for such errors in any antecedently probably way. Trace their causes back far enough, and you will be forced to admit they are always due to arbitrary determination, or chance."[10b.]

Bohr emphasized that it is the finite interaction between the object and the measuring agencies that necessitates a break with classical causality, and in doing this he revitalized ideas that Peirce had formulated more than 40 years earlier; ideas that in Peirce's time were too revolutionary to be recognized as more than crackpot philosophy and had been almost completely forgotten in the meantime.

Bohr, in his philosophy of complementarity and especially in his doctrine of the irreducible quantum jump and the breakdown of classical causality, was probably inspired, if not directly influenced, by the danish existentialist religious philosopher Søren Kierkegaard who around the middle of the 19th century made some highly original contributions to the logic of concepts in theology and psychology. Kierkegaard had an important inspiration in common with Peirce: a fascination of Hegel's dialectical philosophy that later resulted in a thoroughly antagonistic attitude towards it. In several books Kierkegaard emphasizes the freedom of choice as a "qualitative jump" that cannot be analyzed by linear causal reasoning. There is no question af any Hegelian ot Marxian turnover of quantity into quality. In "The Concept of Dread" (1844) he describes the need and the difficulty of another type of bootstrapping circular causality in connection with the jump:

"This jump is furthermore setting the quality, but when the quality is set in the same moment the jump turns into the quality and is preset by the quality. This is an offence to our reason, ergo it is a myth. Accordingly reason itself invents a myth denying the jump and laying out the circle in a straight line, whereby everything proceeds naturally."

In pointing out a possible influence from Kierkegaard to Bohr[11.] the author is well aware that professional philosophers have denied the existence of such a relation[11a.]. Also, that Bohr in his later years had a sceptical attitude towards the entire subject of philosophy. The dominating positivistic philosophy in the 20th century seems to have encouraged the attitude that physical science was shaped by the observation of the facts in nature and not by philosophy. However, Bohr's philosophy of complementarity was made in his younger years when he was not so sceptical and, according to many witnesses, even before the crucial empirical facts of quantum physics were established. On a non-positivistic background as Peircean semiotics or Kierkegaard's conceptual logic it seems not so strange that our language exhibits relational invariances over different subjects and that a psychological problem may have something in common with the "very conditions" for making statements in physics.

Especially after the Aspect experiments it has become generally accepted that the "Copenhagen interpretation" of quantum mechanics stand successfully and unrefuted against its competitors. However, what one calls "the Copenhagen interpretation" is nowadays often confused with a dogmatic faith in the universal validity (completeness) of the formalism and unwillingness to discuss the semantics of alternative interpretations of the wave function. Although, undoubtedly, Bohr in his answer to EPR has contributed to this misinterpretation , it is clearly against his earlier philosophy of complementarity. This epistemology which more than formalism is centered in Copenhagen has never really made its impact on main-stream physics outside this city. Copenhagen in the 19th century was more like a village where everybody knew each other and made far reaching cross-disciplinary ventures uniting the arts with science and philosophy. This is the essence of the "Copenhagen schools" that have emerged in psychology (Rubin), linguistics (Brøndal, Hjelmslev), and physics (Bohr) [11b.], but of course it cannot easily be translated to the international community.

The early success of the Copenhagen school in quantum physics was due to a fruitful mergeance of philosophical ideas resulting in a mathematical formalism. Later, it seems unfortunately, that its proponents (e.g. J. A. Wheeler) have decided to believe in the universal validity of the formalism and extrapolate it ad absurdum in order to dictate a philosophy. The opposite trend, a return to epistemology and a semantic effort to understand the meaning and the limitation of the formalism (e.g. Bell, d'Espagnat, Shimony) is still going on but has found its major inspiration in ideas from outside the circle of Copenhagen, e.g. in Bohm's continuing search for hidden variables. This latter trend has been the driving theoretical force behind the experiments, and it would be a sad fate if the experimental results should drain the energy from it and further a formalistic petrification in the name of Copenhagen.

It is the impression of the present author that the means of formalistic expression are still as inadequate as ever to deal with ideas like Kierkegaard's of the qualitative jump and Bohr's concept of complementarity. The only way to go is back to natural language and forward again with a new and more adequate formalism based on the parts of conceptual logic that have been overlooked and suffocated by the present formalism.

A symptom of the present state of affairs is the way the word "interaction" is used in physical textbooks and journals. A discussion of "interaction" is nowadays considered synonymous with the formalistic task of choosing the suitable Lagrangian or Hamiltonian function. This is rather contrary to the meaning of the word, for in cases where the influence from the environment (the laboratory) on the quantum mechanical system can be described with a Hamiltonian it is presupposed that what Bohr calls "the reaction of the object on the measuring instruments" can be neglected, i.e. there is no interaction but only action of classical fields.

A formalism that operates only with equations of symbolic expressions can never really penetrate behind the idea of classical mechanic causation, so any attempt to solve the measurement problem of quantum mechanics by proposing a new set of equations, perhaps based on fancy Hamiltonians, are doomed from beginning. Kierkegaard and Peirce were aware of the conceptual logic leading to this conclusion long before the dawn of the new physics. For Kierkegaard it resulted in a rather hostile attitude towards all formal systems, whereas Peirce from about 1880 to his death in 1914 was engaged in a grand attempt to formalize his ideas in the framework of relational logic and semiotics. Peirce did never complete his great system (just like Archimedes who never completed physics) and important parts of it are probably still buried in his vast heaps of papers awaiting publication hopefully within the next 10 or 20 years.

The fragments of Peirce's system that have been published have already proved its applicability. One of the most interesting applications in physics of semiotic and synechistic ideas was described by H. M. Paynter in 1961[12.]. Paynter's formalism is based on the primitive notion of energy bonds or interaction-bonds. Physical systems are described with bond graphs exhibiting their structure of interaction relations and very similar to the bond graphs used by Peirce for anlysis of the relational logic of sentences in natural language. The bond graph notation of physics has mostly been applied to problems in engineering and biophysics, i.e. in classical situations where an interaction bond can be translated to two oppositely directed signals whose product is the rate of energy transfer from one conceptual lump of the system to another. However, the basic philosophy of interaction bond graphs is that the concept of interaction is more fundamental than signals and classical causation. We can use bond graphs classically to derive equations of motion, e.g. Hamiltonian, thermodynamic, hydrodynamic equations, or the Schrödinger equation, i.e. all sorts of well known physical equations, but more important, they give us a means of expression behind equations to be used in situations where a translation to equations would miss the point.

4. Categories and triadic relations.

A basic element in Peirce's philosophy is his doctrine of three fundamental categories: firstness, secondness, and thirdness. The following quotation is from his paper "The Architecture of Theories" (1891) [10a.]:

> "Among the many principles of Logic which find their application in Philosophy, I can here only mention one. Three conceptions are perpetually turning up at every point in every theory of logic, and in the most rounded systems they occur in connection with one another. They are conceptions so very broad and consequently indefinite that they are hard to seize and may be easily overlooked. I call them the conceptions of First, Second, Third. First is the conception of being or existing independent of anything else. Second is the conception of being relative to, the concention of reaction with, something else. Third is the concept of mediation, whereby a first and an second are brought into relation".

The aim of Peirce in this paper is to demonstrate that philosophical systems cannot start from scratch and be safely built on "happy thoughts which have accidentally occured to their authors". On the contrary, the philosopher who wishes to build an endurable structure

> "should take note of all the valuable ideas in each branch of science, should observe in just what respect each has been successful and where it has failed, in order that in the light of the thorough acquaintance so attained of the available materials for a philosophical theory and of the nature and strength of each, he may proceed to the study of what the problem of philosophy consists in, and of the proper way of solving it."

Of course the idea of the three fundamental categories is one of Peirce's own "happy thoughts", but he considers it important to show how they crop up in various philosophical systems and sciences. As a very well known example one could take Fichte's dialectical formula:

1. Thesis. 2. Antithesis. 3. Synthesis.

Or we could take an example from chinese philosophy:

1. Yang, the creative. 2. Yin, the receptive. 3. Tao, the way,

with the well known symbol that was adopted by Bohr as his heraldic sign together with the motto: Contraria sunt complementa. Other examples, given by Peirce are:

> "In psychology Feeling is First, Sense of reaction Second, General conception Third, or mediation. In biology, the idea of arbitrary sporting is First, heredity is Second, the process whereby the ac-

cidental characters become fixed is Third. Chance is First, Law
is Second , the tendency to take habits is Third. Mind is First,
Matter is Second, Evolution is Third."

It should be clear from these examples that Peirce uses his three categories ontologically to clarify the essential nature of concepts which he considers as parts af reality. However, his point of origin is not ontology but rather epistemology and the analyses of structures in language and relations of signs. Ideas of ontology cannot be proven logically but are chosen and the best choice is one that acknowledges as real the concepts that are indispensable in the logical, epistemological analysis. The resulting ontology for Peirce is therefore an objectively idealistic realism in sharp contrast with the nominalism that has dominated anglo-saxon empirical philosophy and positivism.

The three categories of Peirce's ontology are direct descendants from his early discovery in the logic of relatives: the fundamental importance of the triadic relations, i.e. relations between three different signs. Earlier logic suffered from the illusion that semantics could be reduced to dyadic relations of two signs, such as a word pointing directly to its object, and accordingly, following Aristoteles, it was mostly occupied with the study of subject-predicate sentences like "the apple is red" and in one-dimensional logical chains that could be classified in a finite family of syllogisms. An important member of this family is the syllogism known as Barbara:

all A is B
all B is C

ergo: all A is C

In the beginning Peirce believed in Barbara as the most important element of reasoning, and his analyses of how other family members could be reduced to Barbara is an important step towards the theory of mathematical quantification which he developed independent of G. Frege. Soon, however, inspired by his friend de Morgan, he realized the limitations of one-dimensional reasoning based on dyadic relations and began to visualize logical and linguistic structures as networks in more than one dimension.

The crucial insight that the triadic relation is fundamental can be formulated as a theorem of discrete topology (analysis situ). A network (a linear graph) where every node has only two incident branches is one-dimensional, i.e. in higher dimensional networks some nodes must have at least three branches. If we consider an electrical network in more than one dimension it can always be deformed to an equivalent structure where every node has at most three branches, and in the dual representations we can similarly reduce the structure such that no mesh has more than three branches. The mathematical description of the topology of an electric network thus reduces to one triadic relation: addition with two operands and one resultant by the application of Kirchhoff's current law (for the nodes) and Kirchhoff's voltage law (for the meshes).

In the more complex case of sentences of natural language we must hunt for genuine triadic relations that cannot be reduced to dyadic relations, says Peirce, and if we encounter relations of more than three signs (words, concepts) then it must be possible to reduce these to triadic relations and thus rewrite the sentence such that the transformed sentence has exactly the same meaning as the original one. This he then demonstrated with examples using the technique of bond graphs.

The most general type of a genuine triadic relation is an asymmetric one such that each of its constituent signs play a special role determined by its place in the relation. In bond graph notation we can depict such a relation as an asymmetric node resembling an idealized concept of an electronic device with three ports: 1. input, 2. output, 3. control. The signs entering the relation are depicted as lines, the bonds, entering the ports:

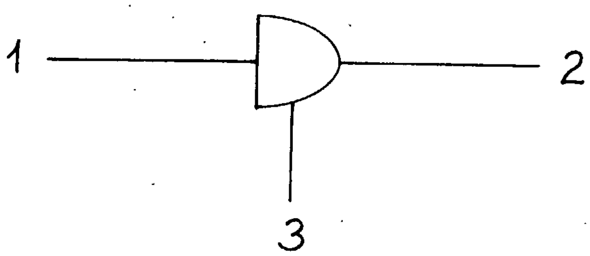


Fig. 1 Bond graph representation of triadic relation.

In this way we also represent the proper relational logic meaning of Peirce's three fundamental categories 1, 2, 3 as "ontologization" of the triadic relation. His idea is that this structure is basic to everything we can speak about and perceive as real, and it is the same structure that applies whether we analyze the most trivial model sentences such as "John gives John to John" or we speak of the highest concepts in theology such as the trinity of christianity.

All relations are relations between signs, hence the close connection between the logic of relatives and semiotics, the theory of signs. But what is then a sign? Peirce says that a sign is something that refers to an object in the general context governed by an interpretant. When we speak of a sign and its meaning we are thus referring to a genuine triadic asymmetric relation, the sign relation, and this is the proto-type of relation of which all other relations are but more or less degenerate copies. Thus we are led into a definitorical circle of signs and relations (as for example when we speak of parts and wholes). It is the same proto-type of relation that defines the three categories, so Peirce can define "a sign" by referring to the categories in the following infamous quotation that probably has scared many away from a closer study of semiotics:

> "A sign, or Representamen, is a First which stands in such a genuine triadic relation to a Second, called its Object, as to be capable of determining a Third, called its Interpretant, to assume the same triadic relation to its Object in which it stands itself to the same Object."

One of the same things Peirce manages to say with these few words is that the interpretant itself is a sign referring to the same object as the primary sign. The strange haunting quality of the formulation above is due to the words "same" and "itself" pointing to a reflexive property of the interpretant. The interpretant must in fact contain a notion of itself as a sign that refers to the same object as the primary sign. The property of reflexivity is very important in Peirce's definitions and he would never have subscribed to Russell's and Whitehead's typology of languages and metalanguages where reflexive sentences are forbidden in order to avoid paradoxes.

The sign relation being a genuine triadic relation can degenerate in three ways towards (almost) dyadic relations, and in this way we can define three types of degenerate signs:

1. Icons, that are signs understood by their own intrinsic properties where the object is nonexisting or "covered" by the sign, e.g. pictures, music.
2. Indices, that point directly to the object by reference or physical contiguity, e.g. a footprint in the sand. Indices are rather context free (the interpretant is withdrawn or absent) but presuppose a real object.
3, Symbols are signs that are just triggers establishing the connection between object and interpretant by convention. Example: a picture of a fish understood to refer to the early christian society of Rome (without the historically delivered convention of interpretation it would be an icon).

These three types of signs are to be regarded as anchorpoints in a continuum of signs in order that an arbitrary sign may be analyzed for its content of iconical, indexical, and symbolic elements. Peirce also introduced other types of classification resulting in 66 different classes of signs, but the three types defined above are the most important. We see them for example in technical diagrams where iconic parts (inductors, resistors, etc.) are clad with symbols (L,R,- -) and indices are used as subscripts (L_1, L_2) to distinguish between variuos objects of the same kind.

Returning now to the discussion of quantum- versus classical physics we can start by depicting the classical notion of causality in the bond graph notation referring to Peirce's categories:

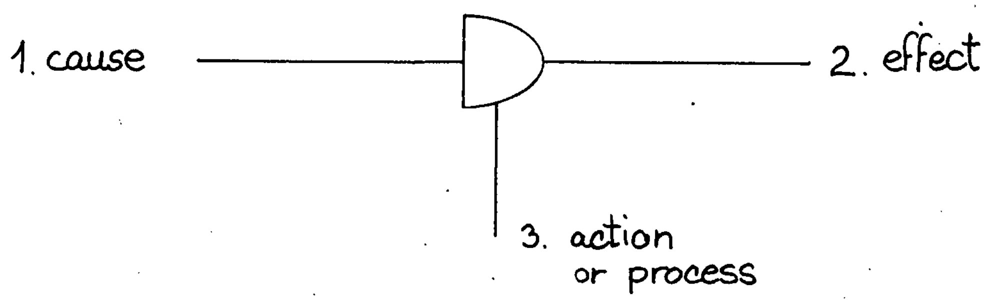


Fig. 2 Classical causality.

In choosing such a pictorial (iconic) representation of abstract concepts we deliberately evoke the association with signal carrying wires and electronic devices. In fig. 2 above the bonds are information bonds each carrying one well defined signal like the voltages of the wires on the front board of a programmed analog computer. We know then that the description of the wires as information bonds is not strictly true: in reality they are energy bonds because physical information cannot be carried without energy transfer. Correspondingly, it cannot only be the voltages that play a role in an analog computer, there must also be a current in the wire in order that a process may develop as a result of interaction between the parts connected with the wire.

In an analog computer the currents are suppressed, i.e. they have to be small in order that the voltages may be considered truly information carrying signals. This is achieved by the operation of active devices hidden behind the panel: the operational amplifiers. We say therefore that energy bonds have been activated, and we can state a general conception of the relational logic of physical systems:

An information bond is an activated energy bond.

An energy bond is then a physical realization of the more general concept of an interaction bond. We can analyze an interaction bond by introducing two indexical signs for the dual concepts of effort (the voltage) and flow, an arrow for the flow and a stroke for the effort together with the convention that the index closest to a system component is considered causally independent of that component (an input variable) but dependent (output) from the system component in the opposite end of the bond: (note that the arrow does not signify the direction of causality but only indicates the orientation convention for the flow).

Fig. 3 Interaction bond connecting two system components A and B with dual indices of effort and flow defining the causality of interaction.

We can then read the interaction causality of fig. 3 in the following way: The effort is output from A and input to B whereas the flow is output from B and input to A. In general we will expect that the output from a system is somehow correlated with the input, so the causal situation is that the input to A (f) influences the output from A (e) which is the input to B influencing the output from B (f) which is the input to A, i.e. a circular causality.

If we could just translate the energy bond to two information bonds carrying signals the two opposite ways then we could also translate the indexical signs to symbolic signal variables having definite numerical values, and if we knew one of these values we could presumably find the other by solving a set of simultaneous equations. This is the normal way to treat interaction bonds for classical situations and it leads back to the causality concept of fig. 2 except in singular cases where "causal conflicts" may arise e.g. if the simultaneous equations have no unique solution. However, this translation cannot in general be strictly valid for physical systems where we consider an energy bond to be a more primitive concept than an information bond. By our thesis above we would then translate one unactivated energy bond to two activated energy bonds which is to explain something simple with something complicated, i.e. it may be a simulation but it is not a translation.

In a quantum mechanical context we must accept that the energy bond is an irreducible concept, and information bonds belong to the classical world although they may influence quantum systems (classical fields). The process of measurement can then be thought of as a means of activation of the energy bonds. In this way we can obtain a synechistic understanding of how our symbolic concepts are related to results of measurement which again are related to indexical signs of nature through a continuous chain of physical and semiotic transformations.

The measurement process regarded semiotically is then the decisive transformation from index to symbol so it must be described with a genuine triadic relation. The bonds entering this relation must be energy bonds, so we cannot translate or explain the structure using the classical concepts of causality. In order to identify the categories we must try to classify the system components in the other end of the bonds, and here we can use the traditional system-science discrimination according to the "degree of activity" of systems which again is an example of the applicability of Peirce's three categories. According to formal system-science as exemplified by Paynter's energy bond formalism we can distinguish between the following three categories of systems:

1. Active systems whose output are independent of the input, i.e. sources of effort or flow.
2. Reactive/passive systems whose output depend deterministically on the previous and the present values of the input. If the previous input values determine the present output we say that the system is reactive, but if the input-output relation is simultaneous we say that the system is passive (this distinction is not so important here). Examples of reactive systems are the storage elements of potential and kinetic energy like capacitors and inductors. Passive behavior we find in reversible two-ports like ideal transformers, transducers and gyrators and in the two triadic elements, the 0-junction and the 1-junction representing respectively Kirchhoff's current and voltage law. All the systems in this category are reversible and it is the only category that is used for modelling a purely mechanical system.
3. Dissipative systems, i.e. sinks of energy like an ohmic resistor. These systems mediate between the two previous categories. In a normal macroscopic context they can be described as almost passive, but according to the fluctuation-dissipation theorem they are always noisy, i.e. there is an active component in their output. Dissipative systems are mediating

in every instance of control: If we want to change the state of a reactive system we will influence it from an active system, e.g. a thermodynamic reservoir, but this will in general produce perpetual oscillations around the wanted state unless dissipative processes bring it to rest.

It should be clear from this discussion that all three of the above categories enter a geniune triadic relation as well for the preparation as for the measurement of a quantum mechanical system. In both cases the source of control acts from the classical world upon the quantum system and the process is mediated by dissipative laboratory equipment. The semiotic discussion of the measurement process is therefore based on the following bond graph relation:

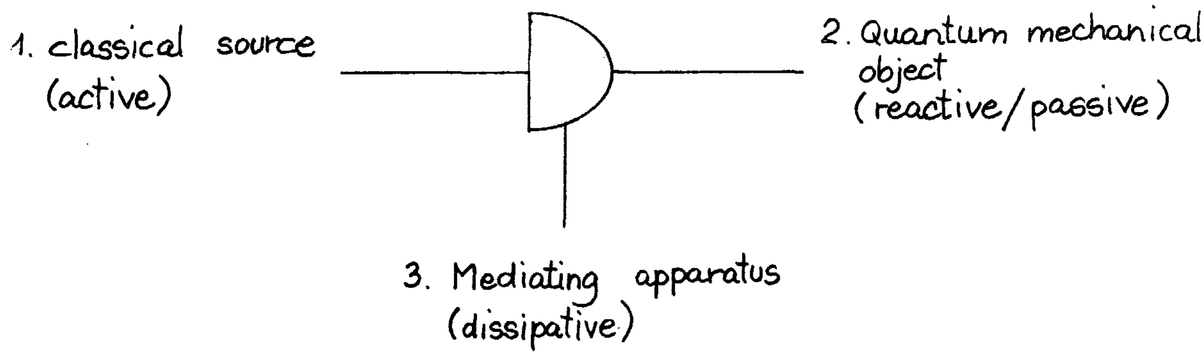


Fig. 4 Bond graph relation of quantum measurement.

By comparison between the classical causal relation of fig. 2 and the measurement relation of fig. 4 we note a certain similarity but also an important difference. In the latter case the bonds are energy bonds, not signal carrying information bonds, and if we want to simulate the process by postulating signals in the bonds then these must be dual signals of opposite directions in each bond and there must be a source of noise associated with the dissipative system according to the fluctuation-dissipation theorem.

It is important to note the difference between an energy bond connected with an active system and an activated bond or information bond. The input in bond 1 to the triadic relation of fig. 4 is determined by an active system, the classical source, but the output in bond 1 ought to carry information if the process considered is to be measurement and it is therefore not suppressed, i.e. bond 1 is not activated. The output information is entering the input port of en amplifier that acts as an activator, i.e. on the other side of the amplifier we find a physical information bond. In the physical information bonds we are allowed to consider one of the dual variables as a classical signal with a well defined symbolic representation, but not the other which is suppressed. This does not mean that the suppressed variable is nonexisting and irrelevant for the discussion, on the contrary, as we shall see in the following sections, but just that it does not possess a symbolic representation. As an indexical sign it exists and plays a role in the process "setting the quality of the jump" as Kierkegaard said and this is because the physical information bonds are an integral part of the classical description of the experimental setup.

Where does then the physical description end? It must end at the precise place where the preset physical information bonds end, i.e. where some permanent mark of registration is made. From there on we can still speak of information transfer through data bonds and semiotic transformations but there is no question of back-action through the data bonds, because the data processing can be postponed untill after the physical measurement process.

We can sum up the discussion with the following bond graph model where the data bonds are shown as dotted lines:

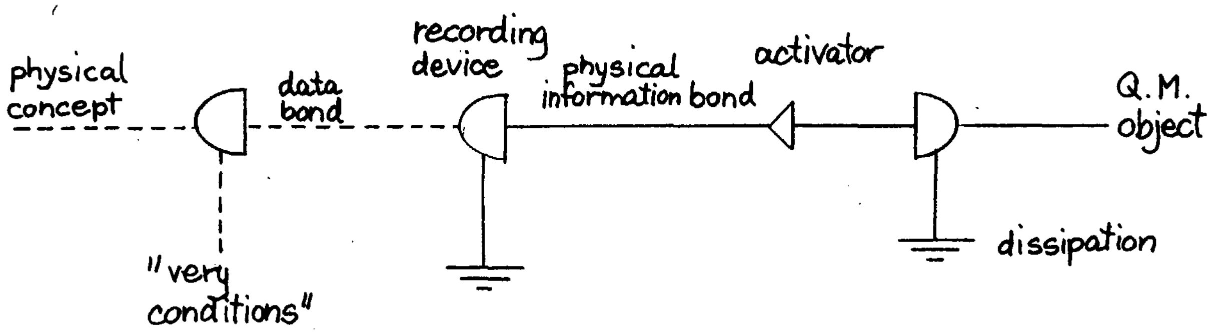


Fig. 5 Semiotic chain of measurement and data processing.

This model should illustrate that the "very conditions" Bohr speaks about lie outside the physical system and can have no influence on the measuring process in a synechistic description. The conditions that have an influence and may affect the quantum mechanical state must be located within the preset system of physical relations and bonds.

5. Zero point noise as a sign of consistency.

Quantum mechanics was born at the turn of the century out of a paradox showing the incompleteness of classical statistical mechanics: The Raleigh-Jeans formula for the spectrum of black body radiation which diverged at high frequences. This divergence, the co called ultraviolet catastrophe, was resolved by Planck by the introduction of the quantum of action. Planck's analysis led to the new formula for the thermal energy of a harmonic oscillator with frequency ν at temperature T:

$$U_{\nu,T} = \frac{h\nu}{e^{h\nu/kT} - 1} \tag{1}$$

an expression that reproduces the classical expression kT for small frequencies but goes to zero exponentially at high frequencies which is fast enough to win over the power law increase (ν^{d-1} in d dimensions) of the phase space factor such that the divergence vanishes.

The subsequent formalistic development showed that there was a term missing in Planck's expression: the zero point energy

$$E_{\nu,0} = \tfrac{1}{2} h\nu \tag{2}$$

It was lucky for Planck and for quantum mechanics that he didn't discover this term, for if he had there would have been no resolution of the ultraviolet catastrophe, but an even more drastic catastrophe would have emerged. The theoretical discovery of the zero point energy came so late that it didn't shatter the faith in the consistency of the new formalism, it was more or less understood that there is a fundamental difference between thermal excitation and zero point motion. The latter is an intrinsic property of every quantum mechanical system related to the Heisenberg uncertainty relations, it cannot be transferred to another system by radiation and it is therefore invisible in the spectrum of the black body radiation.

Although the inclusion of the zero point noise faces us with the disagreeable task of explaining away an infinity of energy in a radiation cavity, at the same time it gives us the benediction of a much more rounded analytical expression for the oscillator energy

$$E_{\nu,T} = E_{\nu,0} + U_{\nu,T} = \tfrac{1}{2} h\nu \coth \frac{h\nu}{2kT} \tag{3}$$

an expression that is an even function of the frequency and matches the classical kT even better for small frequencies than Planck's formula (1). The ability of the quantum formalism to express the balance between absorption and emission of quanta of radiation by means of a single meromorphic function $S(\nu)$ of a complex frequency variable is a profound revelation conditioned by the existence of the zero point energy and indicates that this is not a weakness of the formalism that should be "renormalized" away as quickly as possible but rather a sign of consistency.

In 1928 it was shown by Nyqvist that the problem of thermal noise in a resistor could be reduced to a one-dimensional radiation cavity and that the power spectrum could be expressed by the macroscopic coefficient of resistance without need of any microscopic model of the system. We are allowed to think of a resistor as a collection of independent oscillators whose eigenfrequencies are continuously and evenly distributed, each of them being thermally excited to the mean energy kT. The ultraviolet catastrophe inherent in the resulting spectrum of white noise disappears completely if the resistor is coupled to a linear reactive system like an inductor or a particle with inertia and leaves only the trace of the static classical fluctuation with mean energy ½kT per degree of freedom of the reactive system.

The discussion of classical noise generalized in the fluctuation-dissipation theorem leads directly into the triadic coupling of an active, a reactive, and a dissipative system similar to the one considered in fig. 4. If the active system is a flow-source then the combination of the reactive and the dissipative system (assumed linear) can be described with a frequency dependent complex impedance function $Z(\omega)$ ($\omega = 2\pi\nu$) and the power spectrum of the effort-fluctuations is then expressed by the real (dissipative) part of the impedance function for frequencies on the positive real axis:

$$P_e^T(\omega) = \frac{2}{\pi} Z_1(\omega) \cdot kT \tag{4}$$

An even simpler expression results if we Fourier-transform the impedance function to a time dependent rigidity response-function G(t) and, by the Wiener-Khinchin theorem, introduce the autocorrelation function for the effort variable in the bond connected to the flow-source:

$$\langle e(t')e(t'+t)\rangle_T = kT\, G(|t|) \tag{5}$$

An example of such a triadic coupling of linear systems is shown in fig. 6 as an electric network and in energy bond graph notation:

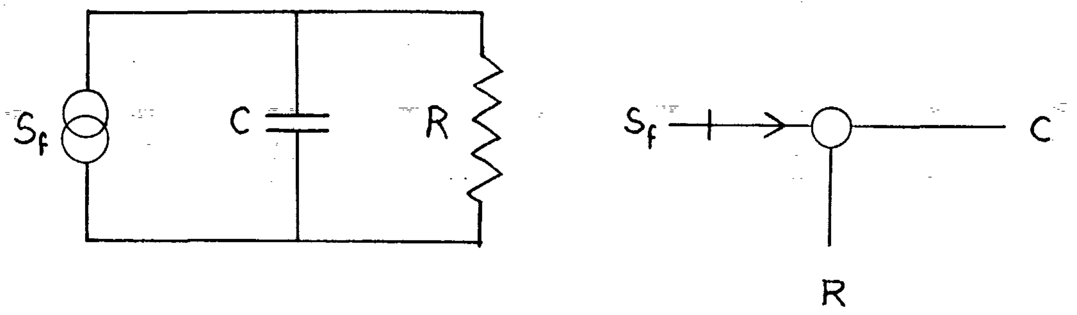


Fig. 6 Example of a triadic coupling of linear systems.

In this case the dissipative part of the impedance function is

$$Z_1(\omega) = \frac{R}{1 + \omega^2\tau^2} \qquad (\tau = RC) \tag{6}$$

and the time dependent rigidity function is

$$G(t) = \frac{1}{C}\, e^{-t/\tau} \qquad (t > 0) \tag{7}$$

In general all three of the system categories enter in the discussion of thermal noise: the active system for identifying the controlled and the noisy variable (viz. flow and effort in fig. 6), the reactive system (the material aspect) for removing the ultraviolet catastrophe, and the dissipative system which ensures the ergodicity (well defined thermal equilibrium) and is the proper source of the noise. In accordance with Peirce's philosophy anything less than a genuine triadic relation of the system categories leads to an improper understanding of the concepts entering the fluctuation-dissipation theorem.

The quantum mechanical generalization of the theorem, made by Callen and Welton in 1951 [13.], results simply in the substitution of the classical oscillator energy in eq. (4) with the full quantum mechanical oscillator energy of eq. (3), i.e.

$$P_e^T(\omega) = \frac{1}{\pi} Z_1(\omega) \cdot \hbar\omega \cdot \coth \frac{\hbar\omega}{2kT} \qquad (8)$$

In this case we cannot by Fourier transformation find a simple proportionality (like eq. (5)) between the time independent rigidity response and the autocorrelation function because the oscillator energy is now frequency dependent, but this is not so serious. The most important new feature introduced by the finite quantum of action is the existence of zero point noise at T = 0:

$$P_e^0(\omega) = \frac{\hbar\omega}{\pi} Z_1(\omega) \qquad (9)$$

The zero point noise exists at all temperatures but is superposed with thermal noise for $T > 0$. Just like in the discussion of black body radiation we must imagine two entirely different sorts of noise: the thermal noise is a "coherent" sort that can be transferred or radiated away to another system of lower temperature, but the zero point noise is non-transferable and intrinsic to the system considered, or rather intrinsic to any genuine triadic relation with an active and a dissipative system the quantum mechanical object may participate in.

The close connection between the zero point noise and the Heisenberg uncertainty relation was pointed out by I. R. Senitzky in 1960 [14]. By considering a damped harmonic oscillator as his model system Senitzky showed that the pure effect of damping could be described with the usual Heisenberg equations of motion by adding an imaginary term proportional to the damping to the Hamiltonian operator of the undamped socillator. This, however, has the unwanted side effect that the displacement-momentum commutator becomes time dependent and relaxes with the same rate as the classical oscillator. In other words: the Hamiltonian description of pure damping contradicts the Heisenberg uncertainty relations and brings us back to a purely classical deterministic theory which is clearly inconsistent. There can therefore be no Hamiltonian theory of damping, and this is because the concept of deterministic damping (dissipation) rests on a false conceptual logic. In order to preserve indeterminacy the damping effect must be balanced with a source of noise and this is just what the fluctuation-dissipation theorem states. The thermal fluctuations are produced by thermal noise, whereas the intrinsic fluctuations in a pure quantum mechanical state are produced by the zero point noise.

The fact that the fluctuation-dissipation theorem including zero point noise follows rigorously from the quantum formalism via response theory (e.g.

a Kubo formula) is a sign of consistency because the zero point noise can be regarded as the source of quantum indeterminacy and thereby an indispensable concept in the semantic discussion of the symbols of quantum mechanics. By accepting the reality of zero point noise in any genuine triadic systems relation we can begin to see how a physical theory based on semiotics may provide a justification and a limitation for Bohr's semantic thesis that the meaning of a wave function lies in the prescription of probabilities in connection with a classically described experimental setup.

6. The qualitative jump.

The semiotic problem of choosing the simplest possible model of measurement is easily solved with reference to the pioneering works of v. Neumann and Dirac. V. Neumann introduced an information theoretical approach by answering the question: how is the "atom of information", the binary question, represented by the quantum mechanical formalism? The answer is that any observable A is represented by a Hermitean operator $\hat{A}$ that can be "spectrally resolved" into a weighted sum of projection operators $\hat{P}_i$ whose only eigenvalues are zero and one:

$$\hat{A} = \sum a_i \hat{P}_i \tag{10}$$

where the a_is are the real eigenvalues of the operator $\hat{A}$. The projection operators form a complete orthogonal system and therefore represent a semantically invariant transformation, a meaning preserving translation of the "question" A to a set of mutually exclusive yes/no questions. If two different observables are represented by the same set of projection operators they are quantum mechanically compatible, i.e. they can be measured simultaneously with the same set of "counters" which means that the physical discussion of measurement can be reduced to the discussion of a single counter (counting only to 0 or 1). This is of course a logical reduction, not a physical one at first blush, but as a physical measurement is an implementation of the logic of observation we should not be surprised to find that a real apparatus at closer inspection reveals itself as a collection of counters. For example: if we measure the position of a particle by means of a photographic plate the measurement is reduced to the counters corresponding to the separate grains of the photographic emulsion. In fact the logic seems to be so compelling that we can safely regard the binary counter as an irreducible component of an ideal measurement in the pragmatic sense of the word, i.e. an idea that governs the manufacturing of actual experimental equipment.

The next step in the discussion is to analyze the operation of a single counter in semiotic terms, i.e. as a physical setting of a sign relation. We have already seen that this consideration leads to the energy bond graph model of fig. 4. This triadic relation is then the crucial link between the symbols of measurement results and the symbols of the quantum mechanical formalism, and in accordance with Bohr's semantic thesis we shall regard it as the place where the meaning of the quantum symbols originates. The use of semiotic terms is valuable in order to provide a general philosophical background but it should not obscure the content of which the main part has been discovered by physicists in the evolution of the quantum formalism. We have already seen that there are close connections between the epistomological ideas of Peirce and Bohr; now we are interested in the more specific development of formalism it is important to note how some of the more formalistic ideas of semiotics emerge in the work of Dirac.

A semioticist would say that the construction of symbolic concepts of physics proceeds via the preliminary sign categories of icons and indices. We start by considering an icon, e.g. the concept of a material particle, then we identify the indexical qualities that can be measured, e.g. inertia, velocity, etc., and finally the conventions of physical standards are usen in connection with a measurement procedure to establish the symbolic concepts. For example the vectorial notation $\vec{v}$ for the velocity of a particle without reference to a coordinate system is indexical in comparison with the symbolic coordinate representation that presupposes the convention of a basis set of orthogonal vectors. Historically the invention of the indexical vector notation came after the symbolical coordinate representation and it seems that the theoretical development has been much retarded by a neglect of a semiotic categorization and an illusory belief in the existence of context-free symbols in nature. Similarly in quantum mechanics: the symbolic concepts of Schrödinger's wave functions and Heisenberg's matrices were invented first and their final theoretical unification in the transformation theory was conditioned by Dirac's invention of an indexical notation: the "bras" and "kets".

The dualistic nature of the bra and ket vector spaces is closely connected with the semiotic idea that the constitutive sign relation of fig. 4 is set by interaction- or energy bonds. In a general interaction (energy) bond we can regard the dual indexical signs (effort and flow) as vectors belonging to dual vector spaces that are connected through the formation of a scalar inner product (the rate of energy transfer). We can distinguish between the two dual spaces by calling them contravariant and covariant, or kets and bras, the meaning is the same and rests in the definition of the inner produkt, the bra-ket $\langle \mid \rangle$. The semiotic concept of the energy bond as the link between the classical and the quantum world is therefore sufficient to relate the indexical signs of the formalism to the concepts of dual vector spaces.

When we describe an isolated quantum mechanical system we can try to put up an equation for the rate of change of a state vector in either of the two dual spaces. In doing so we choose to refer to the state of the system with a sign that is more indexical, i.e. less context dependent, than Schrödinger's wave function, and this is appropriate when we describe the quantum world left to itself. We cannot apply Bohr's semantic thesis to this sign for it has no meaning except its direct reference to the object. The structure of the guiding equation must be reducible to an energy bond graph model (normally an infinitesimal segment that can be repeated in 1, 2 or 3 dimensions). There can be no concepts in the quantum formalism that cannot be brought back to the fundamental concept of the energy bond. The related discovery that the Schrödinger equation and equations of quantum field theory can be translated to electrical network structures was published in 1939 in the monumental monograph of G. Kron [15.]; a similar viewpoint can be found in Brillouin's "Wave Propagation in Periodic Structures".

A general quantum mechanical energy bond represents some n-dimensional subspace of the Hilbert space and it can be decomposed into n elementary bonds, each corresponding to a ray, i.e. a one-dimensional subspace. Every choice of an orthogonal basis for the Hilbert space is then equivalent to the decomposition of a general vector bond into rays. We have seen that an observable and the corresponding apparatus of measurement can be analyzed as a collection of mutually exclusive binary counting operations, so we see that the indexical function of bond 2 in the model of fig. 4 consists in the identification of a ray of Hilbert space. Up to this point the semiotic discussion leads to the same point of view that can be found in other theories, e.g. the quantum logic of Jauch and Piron and the measuring theory of Ludwig[16.].

We can see how a collection of counters serve to establish an orthogonal set of basis vectors for the space of possible state vectors, and these basis vectors have a special physical significance because they are "setting the quality of the jump" i.e. defining the possible outcomes of which only one is realized by the quantum collapse of the state vector during measurement. It has no meaning to speak of a collapse unless these possibilities are set up in advance, but this also means that the preset counters must have a way of making a physical influence on the quantum system before the collapse and there must be a random element in this influence.

Here we must be very careful. It is tempting to think of the dissipative noise that must be inherent in the triadic bond graph model of fig. 4 as a sort of perturbing random Hamiltonian acting on the pure state vector of the object and thereby gradually randomizing its phases such that its density matrix becomes diagonalized to a probability distribution. The idea of gradual phase randomization has been prominent in important theories of measurement, e.g. the theory of Daneri, Loinger, and Prosperi [17.], and by use of the fluctuation-dissipation theorem it could be made to appear very simple and rather convincing. It would also be sufficient in order to point out the important role of the connectedness of the experimental setup thus giving a hint of what makes the quantum formalism valid in the correlation experiments of Aspect and others. But there is something very important missing in such a discription: a diagonalized density matrix is conceptually very different from a probability distribution although its mathematical representation is the same. When we speak of a probability distribution over a discrete set of events we know that just one of these events is realized or going to be realized, but this is not the case when we speak of quantum mechanical density matrix that has been gradually phase randomized to a diagonal form. In the latter case the qualitative jump is missing.

We are looking here at the very problem that Kierkegaard wrote so much about. No matter how hard we try to make a description of the jump in continuoustime we will always miss it. This is because we try to employ ideas of causality that are basically classical to a phenomenon that defies this notion of causality.

When we consider the initial stage of a measurement process before the collapse but after the physical setting of the triadic bond graph relation of fig. 4 we can imagine the existence of virtual small excitations of the bond graph variables as required by the fluctuation-dissipation theorem. We cannot locate the origin of such disturbances in one particular subsystem entering the relation although the convential concept of noise would tend to locate it in the dissipative system and the conventional logic of measurement would tend to locate it in the reactive quantum system. The zero point noise is "non local" in the sense that it is a property of the genuine triadic relation as a whole, not of any of its constituent systems. This does not exclude a simulation of it in strictly local terms, but it should prevent us from attaching any ontological status to such a simulation. The <u>local realism</u> or <u>synechism</u> of our semiotic models has nothing to do with the localizability of causes, for the linear causal thinking is suspended for a while, but it is expressed by the continuity of interaction through space, i.e. by the connectedness of the sign generating bond graph models.

Let us take a close look at the possibilities for simulating zero point noise in connection with the model of fig. 6 in order to see if anything like a qualitative jump might emerge. The noisy variable will in this case be the effort which is the same for all three systems connected in parallel (an 0-junction). As the fluctuation-dissipation-theorem is unable to distinguish between continuous "wave noise"

$$\tilde{e}(t) = \sum_k \tilde{e}_k \exp(-i\omega_k t) \tag{11}$$

and "particle noise" or shot noise

$$e'(t) = \sum_k e'_k \,\delta(t - t_k) \tag{12}$$

and we are looking for a jump, it will be better to use the "particle noise" model of eq. (12).

The dimensions of the bond variables effort and flow can be chosen such that the flow measures the rate of particle transfer, i.e. f has the dimension of a reciprocal time, and the e'_ks in (12) will then have the dimension of action. We can then further "quantize" the model (12) to

$$e'(t) = h \sum_k \eta_k \,\delta(t - t_k) \tag{13}$$

where h is Planck's constant and η_k are random numbers, either +1 or -1. It will still be possible to simulate the arrival times t_k such that the fluctuation-dissipation theorem is satisfied. Of course the continuing noise of eq. (13) is not what we are interested in; we are trying to describe one single event, the first occurring after t = 0, as a model of the qualitative jump. When this has occurred the situation is quite different, some non-linear process has taken place and the linear characteristics used in the construction of the model (13) for the virtual excitations are no longer valid. Thus we shall only use one of the terms in eq. (13) describing the single event, say at $t = t_1 > 0$:

$$e'(t) = h\,\delta(t - t_1) \tag{14}$$

and this single event must be associated with the detection of a particle, i.e. the flow conjugate to e' must be

$$f'(t) = -\delta(t - t_1) \tag{15}$$

Of course the two expressions (14) and (15) cannot be strictly valid, because that would mean an infinite energy transfer at $t = t_1$. The use of delta functions to convey the idea of a sudden jump means that there are two widely separated time scales involved: a micro-time τ_i that characterizes the internal dynamics of the model an a macro-time τ_M that characterizes the whole measurement process. If τ_i and τ_M are separated with many orders of magnitude we can have the macro-appearance of the two simultaneous delta functions, but on the micro-level they will be smooth functions slightly separated in time, like shown in fig. 7. The energy transfer is then finite (but indefinite) and there is always exactly one quantum of action involved in an event.

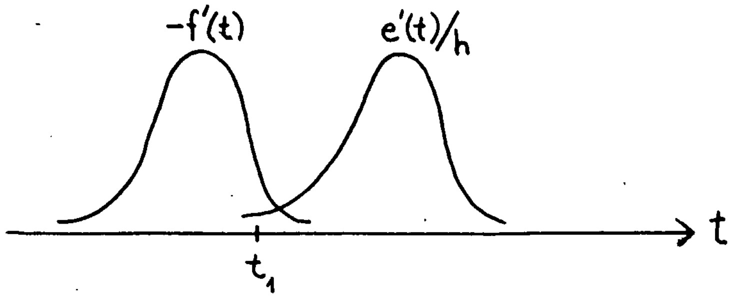


Fig. 7 A Microscopic view of the delta function expressions (14) and (15).

If the model shall be applicable we must then demand that the probability of an event has a nearly constant value for all times on the macroscopic scale, i.e. some orders of magnitude around τ_M , and that this value can be related to the quantum mechanical prescription of probability. For ordinary Poisson-type shot noise this demand can never be fulfilled because the waiting times are gamma-distributed (exponential for the first event) but for zero point noise it is in fact what we find, as we shall now see.

For the model on fig. 6 we can identify the microtime as

$$\tau_i = RC \tag{16}$$

By Fourier transforming the zero point power spectrum (9) we first get the time dependent autocorrelation function and if we integrate this expression twice over time (from 0 to t) and divide it by h^2 we find the following formula giving the average number of events between time 0 and time t:

$$N(t) = \rho \int_0^\infty \frac{1 - \cos \omega t}{\omega \cdot (1 + \omega^2 \tau_i^2)} \, d\omega \tag{17}$$

where ρ is the dimensionless parameter

$$\rho = \frac{R}{\pi^2 h} \tag{18}$$

In the limit $t \gg \tau_i$ we get

$$N(t) \approx \rho\left(\gamma + \ln\frac{t}{\tau_i}\right) = \rho \ln \frac{t}{\tau'} \tag{19}$$

where γ = 0.5772-- is Euler's constant. We see that if t is of the order τ_M, i.e. many orders of magnitude larger than τ_i, N(t) will have a nearly constant value

$$N(t) \approx N_0 = \rho \ln \frac{\tau_M}{\tau'} \tag{20}$$

One may object that the impedance function we have used in (17) still entails an ultraviolet catastrophe for the effort fluctuation. This is true but not important because we are interested in the width function N(t) which is given by a convergent integral. The logarithmic behavior of N(t) for large t is derived from the fact that the impedance has a finite value R for small frequencies and will not be disturbed if we introduce a high frequency cut-off in order to remove the remaining trace of an ultraviolet catrastrophe.

In order to get a more detailed picture we need to determine the waiting time distributions $P_n(t)$, i.e. the probability of having exactly n events in the time interval from 0 to t. We have then

$$N(t) = \sum_{n=0}^{\infty} n\, P_n(t) \tag{21}$$

and the P_ns are determined by the recursive formula

$$P_n(t) = \int_0^t p_1(t')\, P_{n-1}(t-t')\, dt' \tag{22}$$

where

$$p_1(t) = -\frac{d}{dt} P_0(t) \tag{23}$$

is the probability density for the first event. By Laplace transformation

$$\tilde{N}(s) = \int_0^\infty N(t)\, e^{-st} dt \; ; \quad \tilde{P}_n(s) = \int_0^\infty P_n(t)\, e^{-st} dt \tag{24}$$

and use of eq.s (21) - (23) we find the following general formula valid for all types of discrete noise

$$\tilde{P}_0(s) = \left[s\left(s\tilde{N}(s) + 1\right)\right]^{-1}$$
and
$$\tilde{P}_n(s) = \left[1 - s\tilde{P}_0(s)\right]^n \tilde{P}_0(s) \tag{25}$$

so it becomes possible by the inverse Laplace transformation to determine the whole family $P_n(t)$ from the single function N(t) which is derived directly from the fluctuation-dissipation theorem. (A more detailed account of this derivation can be found in the appendix).

The above derivation based on the fluctuation-dissipation theorem for the width function N(t) is based on the assumption that it is possible to define a time homogeneous ensemble describing the noisy system. On the other hand eq.s (21) and (22) assume that the ensemble is selected by the criterion that an event has taken place at t = 0 and the waiting time distributions will in general reflect a non-markoffian memory of the last event. There is no conflict between these two assumptions although it may sound so. The "event" at t = 0 should not be regarded as a real event but only as a selection criterion for a time homogeneous ensemble, the only possible criterion for a non-markoffian discrete stochastic process. The markoffian Poisson-process is exceptional by allowing time zero to be an arbitrary instant between events.

We are not interested in the general solution but only in the special case when N(t) is nearly time independent as described by eq. (20). In this case we can show that

$$P_0(t) \approx \frac{1}{1 + N(t)} \approx \frac{1}{1 + N_0} \tag{26}$$

i. e. the probability of having no event up to time t is also nearly constant on the macroscopic time scale, and similarly for the other functions $P_n(t)$ which will give a <u>geometric</u> distribution for the number of events, n, i.e. quite different from the ordinary Poisson process.

As said before we shall only use the noise model until the first event has taken place which means that the only interesting function is $P_0(t)$ and the probability of having one event is not the $P_1(t)$ of the waiting time distribution family, but instead:

$$\hat{P}_1(t) = 1 - P_0(t) \approx \frac{N_0}{1 + N_0} \tag{27}$$

This is then the expression that should be identified with the quantum mechanical prescription

$$\hat{P}_1 = |c|^2 \tag{28}$$

Where c is the expansion coefficient of the state vector or wave function after the ray of Hilbert space that has been set up by the counter in question. So comparing (27) and (28) we find that N_0 should be

$$N_0 = \frac{|c|^2}{1-|c|^2} \tag{29}$$

which, according to (20) means that the dissipative parameter ρ should have the value

$$\rho = \frac{1}{\ln \frac{\tau_m}{\tau'}} \cdot \frac{|c|^2}{1-|c|^2} \tag{30}$$

At first sight this expression looks a bit artificial, but we shall see that there is a natural explanation for it. The divergence for $|c|^2 = 1$ means that the linear description of virtual excitations breaks down at this value because we here have a transition from probability to certainty of an event. Such a breakdown of a linear response theory is well known, e.g. in mean field theories of phase transitions, and it can be described most generally as the onset of instability of a positive feedback loop. In our model of a counter (fig. 4 and fig. 6) we can find such a transition if we assume that the active system in the relation presents itself to the virtual excitations as a negative resistance $-R_a$. If the passive resistance of the dissipative system is R_p (positive) we get the following expression for the effective resistance

$$\frac{1}{R} = \frac{1}{R_p} - \frac{1}{R_a} \text{ , i.e. } R = \frac{R_p}{1 - R_p/R_a} \tag{31}$$

so we see that the way the dissipative parameter ρ depends on the probability $|c|^2$ follows from the simple ansatz

$$\begin{aligned} R_a &= R' \qquad \text{(a constant)} \\ R_p &= R' \cdot |c|^2 \end{aligned} \tag{32}$$

The constant R' should then have the definite value that satisfies eq. (30), i.e.

$$R' = \frac{\pi^2 h}{\ln \frac{\tau_M}{\tau'}} \tag{33}$$

This then raises the question if a measurement apparatus should always contain a "resistance" that is exactly tuned to the value (33) in order to satisfy the quantum mechanical prescription of probabilities? The answer is no! The value of a resistance depends on the exact symbolic fixation of the energy bond variables, an ideal scaling transformation $e' \rightarrow Te'$, $f' \rightarrow f'/T$ will transform a resistance according to the rule $R' \rightarrow T^2 R'$. This means that the value of the resistance is only well defined after a scale of measurement has been chosen, but this is exactly what we have done in deriving the expression (33). Thus there is a bootstrapping logic involved: the jump is setting the quality, as Kierkegaard said, although it at the same time is preset by the quality.

This again brings us back to Bohr's semantic thesis. The meaning of a quantum mechanical expansion of a state vector after an orthogonal basis

$$|\psi\rangle = \sum_i c_i |i\rangle \tag{34}$$

rests in a classical description of a measurement apparatus. We have provided such a classical description and have seen how it lends meaning to the quantum symbols:

a) By analyzing an apparatus as a collection of binary counters identifying the rays $T|i\rangle$.
b) By using the linear model of fig. 6 in connection with the theory of zero point noise to see how the possibility of the jump emerges.
c) By using the quantum prescription of probability $p_i = |c_i|^2$ to determine the resistance value (33) which fixes the scale of measurement (T) along the ray whereby the numerical value of c_i acquires its meaning.

It may be objected that the model of a measurement apparatus we have used is very crude. This, perhaps, is not so serious. The logic of measurement has here been regarded as belonging to the discipline of semiotics, not to physics proper, and quantum semiotics is concerned with the establishment of quantum mechanical concepts not with detailed physical theories of actual equipment. The models we have used are designed to extract the basic semiotic features of an apparatus, the features that make it possible to set up physical sign relations, and if we have succeeded in this there is no urgent need to investigate more complex models. The "counter" is here described in local terms and if we want a more concrete picture we can think of a single grain or even a single molecule of a photoactive substance in a photographic plate. The process of amplification (development) following the irreversible activation of the molecule is in this case unimportant for the measurement process because it is retrospective, i.e. it is postponed until after the completed measurement.

A much more serious objection is that the meaning of the dissipative constant R' in eq. (33) is unclear. A quantum mechanical system may be regarded as having connections to many different dissipative systems in its environment. In fact it is often said (especially after the Aspect experiment) that "quantum mechanics has taught us to regard the universe as an unbroken whole, that cannot be divided into parts, everything depends on everything else, etc.". We need a discussion of which types of connections are relevant to the measurement problem, and this is the topic of the next section.

7. The concept of connectedness.

The energy bond is an abstract concept used in physical models as an iconical sign for connectedness. The indexical signs of effort and flow that can be attached to it are vectors belonging to dual vector spaces and they can be associated with symbols, i.e. numbers and dimension, only after the choice of a vectorial basis including a measurement prescription and standard. In general a given physical system can be "reticulated". i.e. conceptually structured as an energy bond graph model in many equivalent ways and if a certain basis seems more natural than others then it will also seem natural to choose the energy bonds as simply related to the natural basis vectors. However, what seems "natural" depends on the viewpoint whether we are most interested in a close resemblance with the physical reality as it appears or in theoretical simplicity. It is therefore practical sometimes to have several choices of basis represented iconically within the same model and this can always be achieved by insertion of general tensorial transformers consisting of ideal passive 2-ports and 3-ports in an energy bond. For example, if we consider transmission of electrical energy the reticulation of physical resemblance may contain two energy bonds, because two wires are used, but a theoretically simpler reticulation regards the double-wire as a unit to be described with a single energy bond. The tensorial transformer connecting the two representations will in this case be an 1-junction (a series connection) as shown in fig. 8.

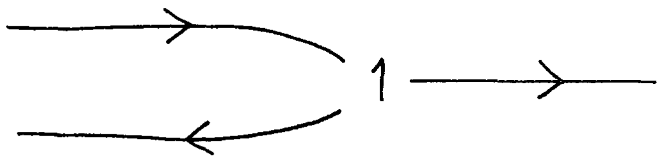


Fig. 8 Reticulation of electrical energy transmission cable as a double wire (to the left of the 1-junction) and as a single cable (to the right).

We can regard the 1-junction in fig. 8 in two ways: either as a dyadic transformer between two different representations of a one-dimensional subspace (a ray) within a higher dimensional space, or as a genuine triadic relation. In the same moment we choose to regard it as a genuine triadic relation we are forced to interpret the three bonds in the same way, as double wires, the two bonds on the left being associated with a common "earth" as illustrated in fig. 9.

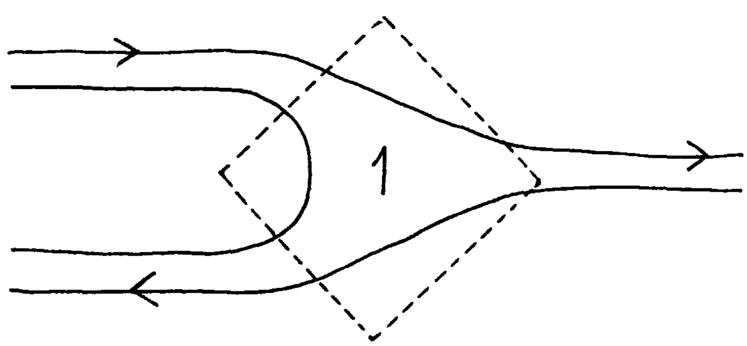


Fig. 9 The diagram of fig. 8 regarded as a genuine triadic relation of double-wire energy bonds.

This example should illustrate that when we have a genuine triadic relation of general vector bonds the three bonds entering the relation must necessarily have the same vector character. If a triadic relation for example involves two n-dimensional vector bonds and one bond of dimension n-1 then it is not a genuine but a degenerate relation in n dimensions and it can be reduced in the simplest cases to a genuine triadic relation in n-1 dimensions and another relation of lower rank, like a mechanical constraint. (For non-holonomic constraints, as for example the rolling of a sphere on a plane it will be necessary to carry the reduction further and it will involve active, monadic relations representing boundary conditions).

The synechistic viewpoint on the measurement problem regards a measurement as a physical setting of a sign relation, i.e. a genuine triadic relation of energy bonds. Furthermore, we have seen that the meaning of a quantum mechanical wavefunction is constituted by the measurement relation, in accordance with Bohr's semantic thesis. This consideration then accentuates the naive viewpoint that was brought forward in sec. 2 in connection with Mermin's exposition of the quantum correlation experiments of Aspect and others: it is the connectedness (which Mermin considers "irrelevant") of the experimental setup that legalizes the theoretical use of the concept of a two-particle wave function, and the "non-local" correlations observed cannot exist if the single particle detectors were as unconnected as Mermin claims they are. There is nothing strange or supernatural in finding two-particle correlations violating Bell's inequality, indeed these are explained very simply by using the concept of collapse of a two-particle wave function. But the explanation is semiotically worthless and non-synechistic if it cannot be documented "in a totally classical way" that the experimental equipment sets up a sign relation of counting where the associated ray of Hilbert space describes a genuine two-particle property. This means that the two distant single-particle detectors must be connected to a central coincidence counter (or another central black box), otherwise the measurement relation degenerates to two independent one-particle relations and the correlations will satisfy Bell's inequality.

The problem is now that "connectedness" is a mathematical abstraction; in the real world everything is connected with everything else, but there are strong connections and weak connections and we need a quantitative criterion in order to decide which connections are relevant in the measurement context and which are not. Intuitively one could say that the plainly visible wires connecting the single-particle detectors with the central black boxes in the polarization correlation experiments must be relevant, but if the wires were removed and the signals sent by radio instead the radio links would be irrelevant connections. But apparently Mermin's intuition (or a hidden rational argument) has led him to the opposite conclusion as can be seen in the passage from his article quoted in sec. 2. Se we need a more substantial argument to justify our intuition.

According to the fluctuation-dissipation theory of the qualitative jump outlined in the previous section a connection is characterized by a dissipative parameter ρ which is proportional to the mean square displacement in a diffusive motion of the phase of the wave function's projection on the ray in question. If we compare connections of different strenght ρ we will find that the time needed before the phase diffusion due to a particular connection of strenght ρ becomes significant varies approximately like $\exp(1/\rho)$ (comp. eq. (19) where N = 1 corresponds to a mean square phase displacement of $4\pi^2$). One special connection will be dominant and setting the quality of the jump, and the jump is setting the scale such that the largest ρ has the value (comp. eq. (33))

$$\rho_{max} = \frac{1}{\ln \frac{\tau_M}{\tau'}} \tag{35}$$

For an ideal measurement the time scales τ_M and τ' are widely separated, so ρ_{max} will be a small number. The exponential dependence on ρ of the characteristic diffusion time for the other connections then makes it plausible that these will not be able to influence the phase significantly during the measurement time τ_M, and thus we find that only one connection is relevant and this is the one set up by the measuring apparatus.

By adopting the dissipative response parameter ρ as the measure of strenght of a connection we can justify the intuitive feeling that the connection established by a radio antenna radiating out into three-dimensional space to some distant receiver is very weak compared to a one-dimensional conductor or wave guide. The dominating resistance felt by a radio antenna is determined by the empty space and not by the receiver, so, if vacuum is an irrelevant connection, so is a radio link. The success of the Aspect experiments in demonstrating that the exact quantum correlations are maintained over distances up to 13 m is a sufficient experimental proof that the vacuum connections and fluctuations are irrelevant under normal laboratory circumstances. Before the last "switching" experiment [4c.] of Aspect the belief expressed by Marshall [8a.] that vacuum fluctuations could communicate the setting of one polarizer to influence the detector at the other polarizer seemed to be a reasonable way of rescuing local realism, but this possibility is now ruled out by the switching experiment.

The earlier conception of local realism was tied up with the ideas of localizability of causes and symbolic hidden variables. The Aspect experiments have given good reasons for abandoning these ideas. But this does not exclude a local realism based on the continuity of interaction, i.e. synechism, with the semiotic logic of signs and relations. With the synechistic concept of local realism, as outlined in this paper, the difference between the non-switching and the switching experiments of Aspect's seems not so important, for the relevant connections are preset and unswitched over the whole experiment in all cases. These are the physical bonds connecting to the place where the final irreversible registration is made in the central black boxes, thus setting the quality of the collapse and the meaning of the wave function. If these connections were removed or sufficiently weakened we would simultaneously remove the very conditions for making predictions using the concepts of a two-particle wave function. If only single particles are detected by independent and unconnected detectors, the only applicable quantum mechanical concept is a weighted superposition of correlated products of single-particle density matrices, and such a construction will always satisfy Bell's inequality, as we shall see in the next section.

Although the synechistic concept of interaction causality has to dispense with the localizability of causes there is nothing to prevent us from simulating situations where signals propagate through the connecting wires. When we claim that a pair of wires leading to a central black box (coincidence counter) is a relevant connection setting up a ray of Hilbert space then we are also opening for the simulation of a process where disturbances originating in the central black box propagate "the wrong way" through the wires and create correlated disturbances at the two separated places where the single particles are detected. Such a simulation should not be regarded as a description of what actually happens in the real event of collapse of the two-particle wave function. The simulation expresses a linear, classical causal logic that is insufficient to grasp the idea of the qualitative jump. Indeed, it would be very unsatisfactory to say that the cause of a detection event lies in the counter, we would rather prefer to say that the cause is the real particles entering the detection chambers. But both descriptions are unsatisfactory because they both rest on the linear causality concept and the qualitative jump of the collapse can only be understood by the circular causal logic of interaction. So. the possibility of simulating signals propagating "the wrong way" is insufficient as an explanation, but it is still necessary to have this possibility, otherwise the application of circular interaction causality would only be an empty postulate.

The question is now: if we consider the set of signals that could possibly travel the wrong way through amplifying devices, is this then an empty set? In the light of the discussion of the qualitative jump in the previous section it is clear that what we must look for are virtual small excitations of shot-noise type. For small signals a linear description will suffice, so what can we think of in linear systems that would permit signal propagation one way, but not the other way? Most linear two-ports are known to satisfy Onsager's reciprocity relations, but we also have antireciprocal linear two-ports called gyrators. Consider the composite two-port in fig. 10 that is a parallel combination (by 0-junctions) of a reciprocal conductance g and an antireciprocal gyrator, also with the magnitude g.

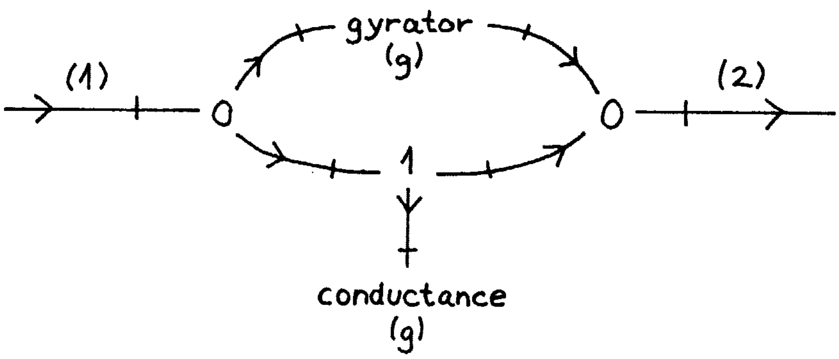


Fig. 10 Linear two-port that allows signal propagation from left to right but not from right to left.

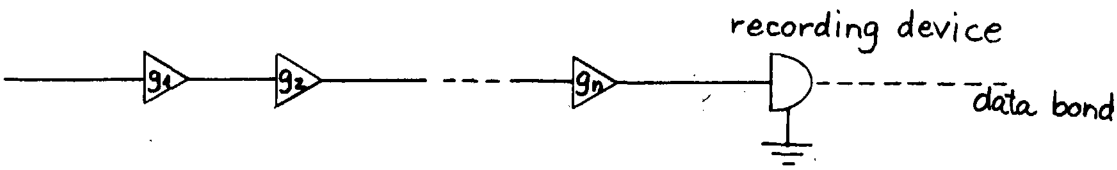
With the flow-orientations chosen from left to right, as shown in fig. 10, the gyrator in the upper branch will have a symmetric response matrix with zeroes in the diagonal, and the lower branch involving the ohmic conductor will have non-diagonal elements of opposite signs, so the response relation of the combined system is:

$$\begin{pmatrix} f_1 \\ f_2 \end{pmatrix} = \left\{ \underbrace{\begin{pmatrix} 0 & g \\ g & 0 \end{pmatrix}}_{\text{(upper branch)}} + \underbrace{\begin{pmatrix} g & -g \\ g & -g \end{pmatrix}}_{\text{(lower branch)}} \right\} \begin{pmatrix} e_1 \\ e_2 \end{pmatrix} = \begin{pmatrix} g & 0 \\ 2g & -g \end{pmatrix} \begin{pmatrix} e_1 \\ e_2 \end{pmatrix} \qquad (36)$$

So we see for this model that an effort signal (e_2) propagating from right to left in bond 2 cannot be felt in bond 1. In fact the model in fig. 10, apart from dual symmetry, is the only possibility of a linear bond graph model that creates the situation of a one-way communication, and if we want to simulate the linear propagation of small signals through a one-way amplifier there can be no path that doesn't go through the simplified device of fig. 10[18].

However, there are good reasons why this device will not work the same way for zero point shot-noise. The dissipative conductance in fig. 10 would give rise to an ultraviolet catastrophe if there wasn't a reactive element hidden in it. This means that there must exist a microscopic characteristic time τ_i such that the lower branch of fig. 10 is a low pass filter that will only allow signals with frequency less than $1/\tau_i$ to pass through. But the simulation of zero point shot-noise is concerned with signals that vary rapidly on the microscopic time scale, so these signals can only go through the antireciprocal gyrator branch of fig. 10. There can therefore be no one-way communication on the microscopic time scale and the zero point shot-noise is passing only through <u>reversible</u>, reciprocal or antireciprocal two-ports.

If we consider a cascade coupling of amplifiers oriented from left to right (fig. 11) and ending in a recording device then the signals propagating the whole way from left to right will be amplified with a cumulative gain-factor $G = g_1 g_2 -- g_n$. Because of the reciprcity or antireciprocity of each of the amplifiers on the microscopic time scale a zero point shot-noise impulse origination somewhere on the line will meet exactly the same gain-factors in the opposite order when it propagates to the left.



<u>Fig. 11</u> Cascade coupling of amplifiers ending in a recording device.

If all the gain-factors g_1, g_2, - - ,g_n are greater than unity it will be the zero point noise originating in the recording device that will be amplified the most on the way to the left end, and this is quite opposite to the situation for classical signals whose leftward propagation is hindered by the bond activating amplifiers.

We see then that bond activation is a classical concept: the more we prevent classical signals from travelling "the wrong way" the easier we make it for the virtule zero point pulses to go just that way. However, this argument only applies to essentially one-dimensional connections and not, for example, to radiation through a two- or three-dimensional space and especially not to the data bonds associated with sign transformations that are not preset but postponed until after the physical process of measurement. Thus, the idea of a "retrospective collapse" that occurs, for example, when the human investigator who has been elsewhere drinking coffee during the experiment comes back and looks at the recorded results, is totally non-synechistic and should be buried as a misconception. The same can be said about the paraphysical ideas of consciousness as an agent acting through non-local hidden variables to affect the collapse. Of course synechism cannot exclude paraphysical effects, but in a physical context where physical synechism works there is no need to resort to paraphysical "explanations".

Having discussed the role of the amplifiers let us consider now the passive web of connections used in a coincidence monitored spin- or polarization correlation experiment. We assume that two particles, 1 and 2, each can be detected in two states denoted ↑ and ↓ as in a spin ½ experiment, but it could just as well be parallel and perpendicular polarizations relative to the respective settings of the polarizers for particle 1 and 2 [4b]. Thus, we have 4 single-particle detectors: 1↑ and 1↓ at polarizer 1, and 2↑ and 2↓ at polarizer 2. There will also be 4 coincidence counters, denoted 1↑2↑, 1↑2↓, 1↓2↑, and 1↓2↓. If the output from a single-detector, e.g. 1↑, is a current pulse it shall be distributed evenly, through a 1-junction, to the two coincidence counters 1↑2↑ and 1↑2↓. On the other side a coincidence counter, e.g. 1↑2↑ shall receive the sum of the current pulses from the single-detectors 1↑ and 2↑ through a 0-junction. The whole scheme of passive connections is then described by the diagram of fig. 12.

We are thinking here of the simplest possible model of a coincidence counter: a parallel connection of a capacitor and a resistor. If it receives two pulses within a relaxation time the charge exceeds a critical value triggering counting whereas a single pulse is insufficient.

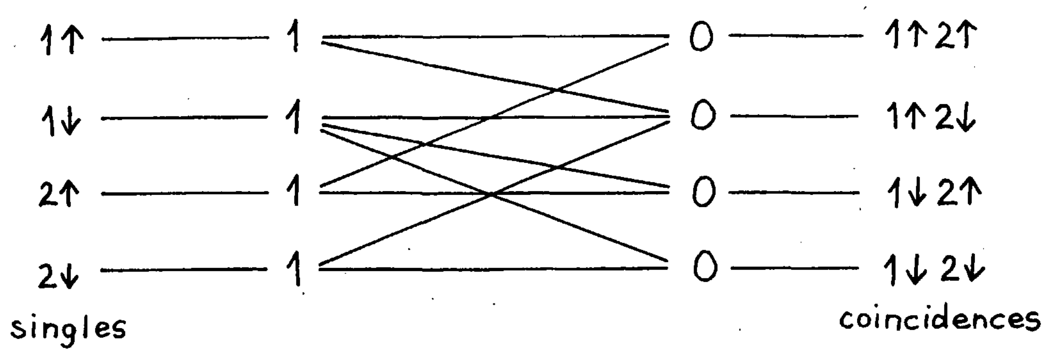


Fig. 12 Passive connections between single-particle- and coincidence counters.

The exact shape of the network of passive connections is not so important for our discussion. The important thing to note is that such a network of energy bond relations always has the reciprocity property, i.e. if the relations ensure that two simultaneous current pulses form the single-detectors 1↑ and 2↑ arrive simultaneously at the coincidence counter 1↑2↑ then they also ensure that a voltage pulse originating at 1↑2↑ will arrive simultaneously at 1↑ an 2↑. The whole network can be regarded as a tensorial transformer inserted in a 4-dimensional energy bond and the rays defined by the coincidence counters can be regarded as a basis for the Hilbert space of the two-particle system.

The simulation approach that forces us to consider the propagation of pulses through wires of considerable length raises some difficult questions. First, it is clear that the picture of two nearly simultaneous delta-function pulses (fig. 7) can only be valid in the immediate neighborhood of the single particle counters but not at the coincidence counters. Should we imagine that a pulse has started at the coincidence counters some time before the detection of a pair of particles and then travelled along the wires in order to arrive just in time for the single detections? Such a model would rely on the notion of backwards causality and is therefore just as strange as models proposed by Wheeler and others. Any physically reasonable model of simulation trying to localize the cause of the collapse must assume that the pulses originate where the single particles are detected. The role of the coincidence counters is then rather to coordinate or synchronize the detection of singles, and we can imagine that this synchronization is established through the previous history of correlated noise that has propagated from the coincidence counters to the single detectors.

The second question is concerned with the uniqueness of the collapse event. We have analyzed a measurement as a collection of independent counters each defining a ray of Hilbert space, and we have seen that the probability of a detection event for a single counter, i, can be described with the quantum mechanical prescription $p_i = |c_i|^2$. But if the counters are independent one should think that the detection events are statistically independent such that there is a finite probability $p_i p_j$ that two different counters, i and j, both register an event even when there should be only one collapse. This would mean that the quantum mechanical probabilities only apply to an ensemble of similarly prepared systems, but not to the single system, as in the interpretations of Kemble, Ballentine and others [19.]. However, cotinuing the line of reasoning, we could consider a situation where the different counters were all connected to a single "selector" that records which of the counters was activated. In this case we would expect that the collapse must be a unique event involving one and just one of the counters.

At present this is just speculation and the author prefers to think that the collapse is unique in all cases, whether there is a central selector-device connecting all the counters or not. After all, the counters are all connected to the real world of quantum phenomena and this may be the only selector we need. For the moment we may leave the question open because it is purely ontological and has no consequence for the experiments we are interested in.

8. The return to reality.

The main purpose of this paper has been to show that Peirce's conception of local realism, i.e. synechism, is compatible with quantum mechanics and that the experimental results obtained by Aspect and others, although they have falsified classical conceptions of local realism based on symbolic hidden variables, are not contradicting quantum semiotics and synechism. We have seen that quantum semiotics gives a theoretical foundation for Bohr's semantic thesis that the measurement process gives meaning to the wave function. On the other hand some of the points in Bohr's reply to EPR are at variance with the synechistic point of view and one cannot from this standpoint say that the very carefully formulated arguments of EPR, which do not depend on hidden variables, have been rejected, neither theoretically, nor by the experiments.

The lack of connectedness that has been emphasized in all the thought-versions, from the original EPR experiment over Bohm's version [20.] opening for the application of Bell's inequalities and to the brillantly papularized version in Mermin's paper [9.], is from the synechistic viewpoint in sharp contradistinction to the strong connectedness via coincidence counters in the real experiments. Therefore, one can say that local realism of the synechistic variety has not been disproved by the real experiments, but the thought experiments for which the Bell inequalities are applicable may still be regarded as possible falsification tests for local realism, provided that their emphasis on the lack of connectedness id taken seriously.

Apparently, the idea that coincidence counters or other central black boxes may have an influence on the correlations measured has not been seriously considered by the authors of the thought experiments. When Mermin, for example, speaks of the lack of relevant connections he is thinking of a way of communication from one polarizer or single-particle detector to the other and not of connections to a central black box. A possible exception to this way of thinking is given by Aspect in his presentation of the idea of the switching experiment [21.]. Aspect considered the possibility that the hidden variables, λ_1 and λ_2, characterizing the two single-particle detectors were statistically correlated (and such a correlation could of course be due to the presence of a central black box), and he then proceeded to show that this correlation would not destroy the validity of Bell's inequalities. The argument, however, has no consequence for our present discussion because it rests on the assumption of local hidden variables, whereas we have seen that sufficiently strong connections to a central black box ensure that the quantum mechanical two-particle wave function is a valid concept.

Quantum mechanics cannot by itself state the exact conditions for the validity of its formalism which depends on a classically described measuring apparatus. In this way quantum mechanics is incomplete, but the incompleteness has nothing to do with hidden variables, and quantum semiotics is in accordance with the Copenhagen interpretation in this view. However, quantum semiotics goes a step further than the Copenhagen interpretation in pointing out the physical connections to the counters as a <u>necessary</u> condition for the reality of the associated quantum symbols. It is still difficult to formulate a <u>sufficient</u> condition which would require a detailed theory of actual measuring equipment and it seems dubious whether such an undertaking can lead to a clear result. Fortunately, there is a lot of experimental evidence showing that the concept of ideal measurements presupposed by quantum mechanics is not an empty postulate. We can feel reasonably confident that the pragmatic logic of experimentalism is precisely what the theory needs.

Here comes then a difficult point: When we propose an experiment, that may either falsify the synechistic theory outlined here or demonstrate the incompleteness of quantum mechanics, then the crucial feature of such an experiment, i.e. a deliberate weakening of the connections between various pieces of equipment, may be regarded as conflicting with a sound experimental praxis. Hence this long exposition that leads to some rather trivial proposals for experiments that could be stated on the back of an envelope. Good experimentalists are naturally proud of their skill and will not voluntarily weaken it unless there are good theoretical reasons for it. Such experiments will only be performed when it is realized by a sufficiently strong group of physicists that the epistomology of quantum semiotics and the ontology of synechism is a possible theoretical standpoint of some explanatory power and thus worth testing.

We shall consider three possible experiments that are all slight modifications of the original photon cascade experiment by Freedman and Clauser [22.] or of the first of Aspect's experiments (Aspect, Grangier, and Roger, 1981) [4a.]. Aspect's experiments have the merit of a long distance (13m) between the single-particle detectors which is desirable in order to ensure that the two photon wavepackets do not overlap in the moment of detection, and thus there is no possibility that one detection should be able to influence the other through a "quantum potential" inherent in the two-particle wave equation. The experiment of Freedman and Clauser has another merit in that it doesn't rely on corrections for accidental coincidences. Both these merits are desirable, but the far most important is that there shall be no question of accidental coincidences. In principle it should be easy to fulfil this demand by decreasing the counting rate of single photons sufficiently.

The diagram for the experiment is shown in fig. 13. The connections to the coincidence counter are shown with dotted lines. The three proposals we shall consider are just three different ways of weakening these connections.

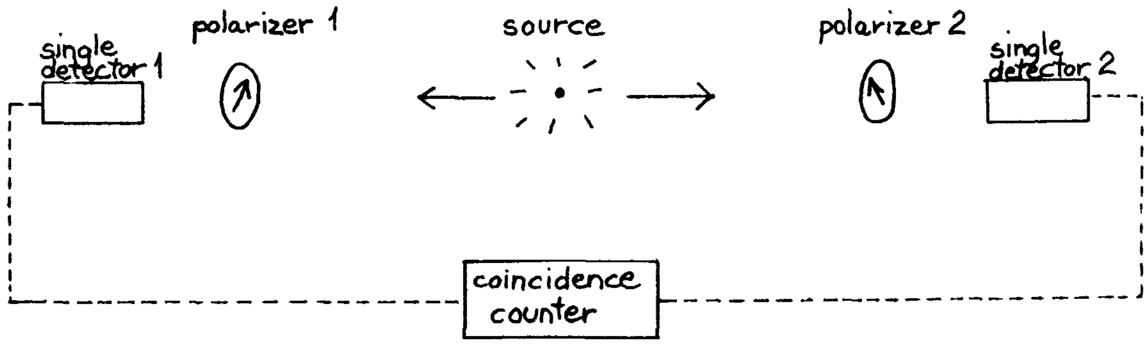


Fig. 13 Diagram of photon cascade experiment.

We shall not go into a detailed discussion of polarizer- and counter efficiencies etc. which can be found in the review article by Clauser and Shimony [3.], so we assume that the polarizers are ideal and that the "no enhancement" hypothesis of Clauser and Horne [23.] is valid. The fraction of coincidences as a function of the angle θ between the polarizers is then according to quantum mechanics

$$G_{c,q}(\theta) = \frac{N_c(\theta)}{N_\infty} = \frac{1}{2}\cos^2\theta \tag{37}$$

where N_∞ is the number of coincidences in the absence of polarizers.

Let us for a moment assume that a certain fraction γ were accidental coincidences. This would weaken the correlation such that the expression (37) should be replaced by

$$G_{c,\gamma}(\theta) = \frac{1}{4}\gamma + (1-\gamma)\cdot\frac{1}{2}\cos^2\theta \tag{38}$$

If we had no way of identifying the fraction γ as accidental coincidences and found something that could be described with the expression (38) then we would have to accept it without correction. Bell's inequality will then be satisfied if

$$\gamma > \gamma_B = 1 - \frac{1}{\sqrt{2}} \simeq 0.29 \tag{39}$$

In Aspect's experiment the correction factor γ is about 0.4 so the resulting disagreement with Bell's inequality is strongly dependent on an explicit correction for accidental coincidences which were measured directly by a delayed coincidence counter and then subtracted from the total coincidence rate. This procedure was recently criticized in an interesting paper by E. Santos [24.].

If the experiment were performed without connections to central black boxes it would be impossible to make a direct counting of accidental coincidences. It would then be necessary to have a sufficiently low counting rate of single photons such that the fraction of accidental coincidences could be considered negligible from statistical reasons alone. If then an expression of the form (38) was measured, with $\gamma > \gamma_B$, it could only be explained by admitting that the two-particle wave function collapse was nonexisting and that only single particle measuring events were taking place.

According to the present theory the conjecture $\gamma > \gamma_B$ would be the weakest bid for the outcome of such an experiment. A much more reasonable prediction is $\gamma = \frac{1}{2}$, as we shall now see. Imagine first that the two photons were in a pure state where both photons are polarized in a direction $\vec{n}$ having an angle u with the vertical. If the direction of polarizer 1 is vertical the amplitude for a detection of both particles would be

$$\langle 1\parallel, 2\parallel \mid 1\vec{n}, 2\vec{n} \rangle = \cos u \cdot \cos(u-\theta) \tag{40}$$

If the density matrices corresponding to such pure states are averaged with equal weight over all angles u we find the following expression replacing (37)

$$G_{c,s}(\theta) = \frac{1}{2\pi} \int_0^{2\pi} \cos^2 u \cdot \cos^2(u-\theta)\, du = \frac{1}{8} + \frac{1}{4}\cos^2\theta \tag{41}$$

i.e. of the form (38) with $\gamma = \frac{1}{2}$.

This model that is based on a superposition of products of single-particle density matrices has been proposed several times. According to the Furry-hypothesis [25.] a two-particle wave function would spontaneously degenerate into such a sum when the spatial wavepackets of the two particles no longer overlapped. This hypothesis vas disproved by the Aspect experiment. According to the present theory there is no spontaneous degeneration of the pure two-particle state; the effect leading to (41) should rather be considered a sort of <u>coarse graining</u> due to a measuring equipment that because og its lack of connectedness is unable to define a two-particle collapse. The angle u plays now the same role as a hidden variable in Bell's theory, so any probability distribution for the facorized density matrices will lead to Bell's inequalities, But it is difficult to argue for any distribution except the uniform one that leads to the expression (41).

Let us now consider three different ways of weakening the connections to the central black box.

The <u>first proposal</u> is simply to remove any preset connections and coincidence counters (or time-to-amplitude converters, etc.). This would mean that the coincidencs have to be found retrospectively by comparison of sufficiently accurate time records of the detections of single particles. The diagram in fig. 13 is still applicable if we regard the connections shown as dotted lines ad data bonds. Retrospective coincidence counting has been used earlier, notably in the famous experiment by Bothe and Geiger [26]., but, to the knowledge of the author, never in cases where there is a conflict between orthodox quantum mechanics and a local realistic theory.

The second proposal is to use a preset coincidence counter with wireless radio links to transmit the single events instead of wires. This would be a test of the hypothesis put forward in the previous section that the relevant connections in an experimental setup are essentially one-dimensional and not radiation through higher dimensional spaces. Other modifications are possible by inserting a "weak link" in a wired connection one might be able to study a transition from the strongly connected case described by (37) to the unconnected case described by (41).

We can imagine a weak link in a one dimensional connection as a place where a package of information is formed and has to propagate further by pure diffusion (unlike the diffusion over the base layer of a transistor where the inertia of the injected particles is essential). Similarly, if the information package propagates like a soliton (e.g. a nerve pulse) or like a mailed letter. In such cases the information package has the character of a permanent record and its line of propagation has the semiotic character of a data bond without back action.

The third proposal is to keep the strong preset connections to a coincidence counter but to introduce an asymmetry in the placement of the source and to compensate for this by a sufficient delay in one of the connections to the coincidence counter. Until now we have tacitly assumed that the source is placed exactly midway between the two polarizers and single-particle dedetectors as has been the case in most of the experiments. Let us now assume that the source is displaced a piece x towards polarizer 1 such that there is a difference $\tau_x = 2x/c$ in the time of flight of the two photons. It is then necessary to compensate for this difference by inserting a passive delay τ_x in the wire from single-detector 1 to coincidence counter (e.g. by using a longer wire). We have seen that the logic of the collapse depends on the possibility of simulating a pulse that propagates from the coincidence counter to the single-particle detectors. Now such a pulse will arrive the time τ_x later at 1 than at 2, but the wavepacket of photon 1 arrives τ_x earlier than that af photon 2 and the wavepacket has a limited temporal extension τ_w, so if

$$\tau_x > \tau_w/2 \,, \quad \text{i.e.} \quad x > x_c = \frac{1}{4} c\tau_w \tag{42}$$

there will be no possibility for the pulse to be there when both wave present and thus no possibility for a two-particle collapse. With the value τ_w = 5ns that is relevant for the cascade process used in the experiments we find $x_c \approx$ 40cm. If the distance from the midpoint to the polarizers is 6.5m as in the Aspect experiment there should be ample space to study the transition from the regime of eq. (37) ot that of eq. (41) by gradually increasing x from 0 to a value above x_c.

The argument above leading to a rather small value of x_c is perhaps too primitive because it relies on the concept of a single pulse propagating from the coincidence counters, and as we have seen in the previous section, if such a pulse should be responsible for the collapse we should accept the notion of backwards causality. The role of the coincidence counters is rather to synchronize the single detections within the window of coincidence and it seems therefore a more reasonable bid for the critical distance x_c if we substitute τ_c, the coincidence window, for τ_w in (42). In the Aspect experiments τ_c= 18ns which increases our estimate of x_c to 135cm. There will still be ample space and the previous estimate (40cm) retains its significance as a lower limit for x_c because the temporal extension of the wavepacket, τ_w, is a lower limit for the window of coincidence, τ_c.

An asymmetric placing af the source was tried in the experiment by Faraci et al. [27]. Their conclusion was for the case of greatest asymmetry that the result was in better agreement with a local realistic theory than with orthodox quantum mechanics. However, the uncertainties are too big to allow a conclusion to be drawn from this single result.

The three proposals have been ordered according to priority. The first is the clearest falsification test of the synechistic theory outlined in this paper. If it gives results in accordance with orthodox quantum mechanics after a retrospective data processing without any correction for accidental coincidences then one can safely conclude that this theory is wrong. On the other hand, a result in accordance with (41) would be a clear proof of the incompleteness of quantum mechanics and a good point in favor of the synechistic conception of local realism. The second and the third proposed experiments would not be relevant if synechism fails in the first test, but they are probably easier to perform, especially the second, and they can also be regarded as falsification tests, perhaps not as much for the basic viewpoint of synechism but rather for more specific points in the present application of synechism and semiotics.

The author believes that the formalism of quantum mechanics when interpreted according to the semantic thesis of Niels Bohr is consistent but incomplete, like mathematics, according to Gödel. Local realism in its synechistic formulation (continuity of interaction) is compatible with all known dynamical equations and seems to contain so much explanatory power that the incompleteness of the quantum formalism is likely to show up in cases where the formalism contradicts local realism. This is again the EPR argument, but with the synechistic concept of local realism there is no need to look for hidden variables as a more basic description of reality. On the contrary: The application of Peirce's theory of semiotics in connection with the energy bond formalism of Paynter points to the applicability of the quantum formalism in cases where the necessary physical sign relations are preset by the measuring apparatus. There seems to be a hidden logic of good experimentalism that normally ensures the validity of the quantum formalism and quantum semiotics is an attempt to reveal this logic by pointing to the importance of preset connecting bonds. If the necessary connections are absent or weak a coarse graining procedure must be used instead of predictions based on the collapse of the pure state. In the case of two-particle spin- or polarization correlation experiment the coarse graining needed in the absence of preset connections to a coincidence counter will lead to the applicability of Bell's inequalities.

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APPENDIX

Simulation of discrete noise.

The fluctuation-dissipation theorem (8) gives by Fourier-transformation (Wiener-Khinchin theorem)

$$\langle e(t')e(t'+t)\rangle_T = \int_0^\infty P_e^T(\omega)\cos\omega t\, d\omega = \frac{\hbar}{\pi}\int_0^\infty Z_1(\omega)\cdot\omega\coth\frac{\hbar\omega}{2kT}\cdot\cos\omega t\, d\omega \quad \text{(A1)}$$

We now introduce the phase varable ϕ by the definition

$$\phi(t) = \frac{1}{\hbar}\int_0^t e(t')\,dt' \quad \text{(A2)}$$

This variable will perform a diffusive motion with the width function

$$\begin{aligned}\langle\phi(t)^2\rangle_T &= \frac{1}{\hbar^2}\int_0^t dt'\int_0^t dt''\langle e(t')e(t'')\rangle_T \\ &= \frac{2}{\pi\hbar}\int_0^\infty Z_1(\omega)\coth\frac{\hbar\omega}{2kT}\cdot\frac{1-\cos\omega t}{\omega}\,d\omega\end{aligned} \quad \text{(A3)}$$

For shot noise of the type (13) we find that the average number of events from time 0 to t is given by the width function divided by $4\pi^2$, i.e.

$$N(t) = \frac{1}{\pi^2 h}\int_0^\infty Z_1(\omega)\coth\frac{\hbar\omega}{2kT}\cdot\frac{1-\cos\omega t}{\omega}\,d\omega \quad \text{(A4)}$$

so, for T = 0 when we use (6) for $Z_1(\omega)$ we get eq. (17). and for arbitrary T we have

$$N(t) = \rho\int_0^\infty \frac{1-\cos\omega t}{\omega(1+\omega^2\tau_i^2)}\coth\frac{\hbar\omega}{2kT}\,d\omega \quad \text{(A5)}$$

with $\rho = \frac{R}{\pi^2 h}$.

For general time-homogeneous shot noise the connections between N(t) and the family of waiting time distributions $P_n(t)$ are given by eq.s (21) - (23).

By transforming these equations with the Laplace transform (24) we get

$$\tilde{N}(s) = \sum_{n=0}^{\infty} n\,\tilde{P}_n(s)$$

$$\tilde{P}_n(s) = \tilde{p}_1(s)\,\tilde{P}_{n-1}(s) \qquad \text{(A6)}$$

$$\tilde{p}_1(s) = 1 - s\tilde{P}_o(s)$$

and accordingly

$$\tilde{P}_n(s) = \left[1 - s\tilde{P}_o(s)\right]^n \tilde{P}_o(s) \qquad \text{(A7)}$$

$$\tilde{N}(s) = \tilde{P}_o(s)\sum_{n=0}^{\infty} n\cdot\left[1 - s\tilde{P}_o(s)\right]^n = \frac{1 - s\tilde{P}_o(s)}{s^2\tilde{P}_o(s)} \qquad \text{(A8)}$$

Solving eq. (A8) for $\tilde{P}_o(s)$ we get

$$\tilde{P}_o(s) = \frac{1}{s\left[s\tilde{N}(s) + 1\right]} \qquad \text{(A9)}$$

Let us first consider the classical limit

$$kT \gg \hbar/\tau_i \quad , \qquad t \gg \tau_i \qquad \text{(A10)}$$

where we get from (A5)

$$N(t) = \frac{2RkT}{\hbar^2}\cdot t = t/\tau_c \qquad \text{(A11)}$$

showing that this is a markoffian process with the time-independent intensity $1/\tau_c$. Laplace transformation gives

$N(s) = 1/(s^2\tau_c)$, so, by (A9): $\tilde{P}_o(s) = \dfrac{\tau_c}{1 + s\tau_c}$

and by inverse Laplace transformation

$$P_0(t) = \frac{1}{2\pi i} \int_{\Sigma - i\infty}^{\Sigma + i\infty} \tilde{P}_0(s)\, e^{st}\, ds = e^{-t/\tau_c} \tag{A12}$$

The other distributions $P_n(t)$ are found to be the gamma distributions characterizing the ordinary Poisson process. This is all very well known and should just illustrate that the general formalism based on (A5) and (A9) works in the classical limit but that the result (A12) never approaches a constant finite value and therefore cannot reproduce the quantum mechanical probability for the qualitative jump.

For the zero point noise we can determine N(t) directly from eq. (17) and find it analytically expressed by exponential integrals

$$\frac{1}{\varrho} N(t) = \frac{1}{2}\left[e^{t/\tau_i} E_1(t/\tau_i) - e^{-t/\tau_i} Ei(t/\tau_i)\right] + \gamma + \ln\frac{t}{\tau_i} \tag{A13}$$

where γ is Euler's constant. In the two limits $t \ll \tau_i$ and $t \gg \tau_i$ we have

$$N(t) = \begin{cases} \frac{\varrho}{2}\frac{t^2}{\tau_i^2}\left(\frac{3}{2} - \gamma - \ln\frac{t}{\tau_i}\right) & \text{for} \quad t \ll \tau_i \\ \varrho\left(\gamma + \ln\frac{t}{\tau_i}\right) = \varrho \ln\frac{t}{\tau'} & \text{for} \quad t \gg \tau_i \end{cases} \tag{A14}$$

By Laplace transformation of eq. (17) we find

$$\tilde{N}(s) = \varrho \cdot \frac{\ln\frac{1}{s\tau_i}}{s\cdot(1 - s^2\tau_i^2)} \tag{A15}$$

and hence, from (A9)

$$\tilde{P}_0(s) = \left[s\left(\varrho\frac{\ln\frac{1}{s\tau_i}}{1 - s^2\tau_i^2} + 1\right)\right]^{-1} \tag{A16}$$

When using this expression in the inverse Laplace transform (A12) we let be an infinitesimal positive quantity, i.e. s = $-i\omega + 0+$, and

$$\ln \frac{1}{s\tau_i} = \ln \frac{1}{|\omega\tau_i|} + i\frac{\pi}{2}\,\mathrm{sign}(\omega)$$

so that $P_o(t)$ can be calculated by the real integral

$$P_o(t) = \int_0^\infty \frac{(1+\omega^2\tau_i^2)\left[\frac{\rho}{2}\cos\omega t + \frac{1}{\pi}(\rho\ln\frac{1}{\omega\tau_i} + 1 + \omega^2\tau_i^2)\sin\omega t\right]}{\omega\left[(\rho\ln\frac{1}{\omega\tau_i} + 1 + \omega^2\tau_i^2)^2 + \frac{\pi^2}{4}\rho^2\right]}\,d\omega \qquad \text{(A17)}$$

which can be determined numerically for all values of ρ . It will always decrease monoticall from $P_o(0) = 1$ to $P_o(\infty) = 0$.
As shown in sec. 6 the ideal case is characterized by a small value of ρ of the order $1/\ln(\tau_M/\tau_i)$ because N(t) shall have nearly constant value of the order unity when t is of the order $\tau_M \gg \tau_i$. We can estimate ρ in the following way: The microtime τ_i is about h/E where $E \approx 1\text{eV}$ is a suitable activation energy, i.e. $\tau_i \approx 10^{-15}\text{s}$. Putting $\tau_M \approx 10^{-9}\text{s}$ we find

$$\rho \lesssim 0.1 \qquad \text{(A18)}$$

For such small values , $P_o(t)$ will be nearly constant in a large interval around τ_M.

In order to find $P_o(t)$ in the appropriate limit we put

$$\rho = \frac{q}{\ln\frac{\tau_M}{\tau'}} \qquad \text{(A19)}$$

so, by letting $\tau_M \to \infty$ for fixed q and τ' we find: $P_o(t) = C(\xi) + S(\xi)$ where $\xi = t/\tau_M$ and $C(\xi)$ and $S(\xi)$ are respectively the cosine- and the sine-part of the integral (A17)

$$S(\xi) \approx \frac{1}{\pi(1+q)}\int_0^\infty \frac{\sin u\xi}{u}\,du = \frac{1}{2(1+q)}$$

$$C(\xi) \approx \frac{\rho}{2}\int_0^1 \frac{du}{u(1+q+\rho\ln\frac{1}{u})^2} \approx \frac{1}{2}\int_0^\infty \frac{dz}{(1+q+z)^2} = \frac{1}{2(1+q)}$$

i.e. $P_o(t) \simeq \frac{1}{1+q}$

This approximation can also be used in the limit $t \gg \tau_i$ if we, instead of defining q by (A19) use the slowly time-dependent function $q = N(t)$, so

$$P_o(t) \approx \frac{1}{1+N(t)} \qquad \text{(A20)}$$

and for the other distributions $P_n(t)$ we find

$$P_n(t) = \frac{1}{1+N(t)} \cdot \left[\frac{N(t)}{1+N(t)}\right]^n$$

showing that in this limit we have a geometric distribution for the number of events, quite different from the Poisson distribution in the classical limit.

For the case $\rho = 1$ the integral (A17) has been evaluated numerically. The following interpolation formula gives the result to 4 decimals ($x = t/\tau_i$):

$$P_o = \begin{cases} \exp\left\{-x^{1.75}(-.0276x^3+.1858x^2-.5375x+.9598)\right\} & \text{for } 0 \leqslant x \leqslant 1 \\ 2.2088x^{-5}-7.3107x^{-4}+8.2578x^{-3}-3.4264x^{-2}+.6146x^{-1}+.2155 & \text{for } 1 < x \leqslant 5 \\ (\ln x+3.9256x^{-3/2}-3.0034x^{-1}+1.7651x^{-1/2}+1.7511)^{-1} & \text{for } x > 5 \end{cases}$$

The interpolation formula can easily be inverted on a programmable calculator and the stochastic process can then be simulated in the following way:

a) We choose an $x = t/\tau_i$ and calculate $u = P_o(x)$.

b) We generate a random number v with a uniform distribution between 0 and 1.

c) If $v < u$ the number of events es zero (simulation over).

d) If $v > u$ we determine $x_1 = P_o^{-1}(v)$, and x_1 is then the time of the first event.

e) We then go back to a) using x_1 as the new origin of time and $x - x_1$ as the new value of x. This procedure continues summing the number of events until it ends in point c).

The following table gives results for 100 simulations with $x = 10$ ($\rho = 1$).

no. of events:	0	1	2	3	4	5	6	7	8	9
no. of simulations:	28	16	13	11	10	8	5	6	2	1

Average: $\langle n \rangle = 2.52$ (theory $N(10) = 2.5363$). Std. dev. $\sigma_n = 2.41$.

The fact that the standard deviation σ_n is nearly equal to the mean value $\langle n \rangle$ indicates a geometric process. So, we see that even though $\rho = 1$ is not a particularly small value we find a nearly geometric distribution for the number of events.

Notes and references.

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2. J. S. Bell, Physics 1, 195 (1965)

3. The following review article gives a thorough discussion of the experimental implementation of Bell's theory:
J. F. Clauser and A. Shimony, Rep. Prog. Phys. 41, 1981 (1978).

4.a A. Aspect, P. Grangier, and G. Roger, Phys. Rev. Lett. 47, 460 (1981)
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d A. Aspect and P. Grangier, Proc. Int. Symp. Foundations of quantum mechanics, Tokyo, 1983, pp 214 - 224.

5. N. Bohr, Phys. Rev. 48, 696 (1935).

6. See for example: R. C. Lyndon, "Notes on Logic". D. van Nostrand, Princeton 1966.

7. L. Rosenfeld who acted as a secretary and discussion partner for Bohr has told the following story of the reaction to the EPR-article in the memorial volume "Niels Bohr" (1964). (My translation from danish).

"As soon as Bohr had heard my account of Einstein's argument everything else was put aside: we had to clear up such a misunderstanding at once. We should answer by going through the same example and demonstrate the correct way of speaking about it. Very anxious Bohr immediately began to dictate me a sketch for such an answer, but soon he became hesitant: "No, this will not work. We'll have to try once more from scratch ... We'll have to make it totally clear ...". In this way we continued for some time increasingly puzzled by the unexpected sophistication of the argument. Now and then he turned to me: "What can they mean? Do you understand it?" Some rather unconvincing attempts of interpretation followed. Evidently we were farther from the goal than we had first believed. Finally Bohr stopped with the well known remark that he "had to sleep on it". The next morning he continued the dictation immediately and it struck me that there was a change in the sound of sentences. Yesterday's sharp expression of disagreement had vanished. As I remarked to Bohr that he now seemed to consider the case more mildly he smiled: "That is just a sign," he said, "that we are beginning to understand the problem". And really, now the serious work began. Day after day, week after week the whole argumentation was patiently investigated by means of simpler and more transparent examples. Einstein's problem was reshaped and its solution formulated again with such precision and clarity that the weakness in the reasoning of the critics became evident, and their whole argumentation, in spite of all its fake spirituality, was shattered to pieces. "They do it nicely," was Bohr's comment, "but what counts is to do it correctly".

8. Compare, for example, the following two papers by T. W. Marshall, before and after the "switching" experiment by Aspect, Dalibard, and Roger [4c.]:

a T. W. Marshall, Phys. Lett. 75A, 265 (1980) (with the title "The Aspect experiment and the return to reality", comp. sec. 8. of this paper. Marshall writes here about the role of the zero point vacuum fluctuations).

b T. W. Marshall, E. Santos and F Selleri, Phys. Lett. 98A, 5 (1983). (Here the critique is directed against the "no enhancement" hypothesis of Clauser and Horne [17.]).

9. N. D. Mermin, Am. J. Phys. 49, 940 (1981).

10. A good introduction to Peirce's philosophy can be found in the following 5 papers he wrote to "The Monist", 1891 - 93:

a C. S. Peirce, The Monist I, 161 (1891).

b - - - - - - - , ibid. II, 321 (1892)

c - - - - - - - , ibid. II, 534 (1892)

d - - - - - - - , ibid. III, 1 (1893)

e - - - - - - - , ibid. III, 176 (1893).

Abbreviated versions of these papers can be found in:

f J. Buchler (ed.) "Philosophical writings of Peirce", Dover, N.Y. (1955). Other sides of Peirce's work in: (Pragmatism, semiotics, etc.)

g "Collected Papers, C. S. Peirce", vol. I - VIII. Ed. Charles Hartshorne and Paul Weiss, The Belknap Press of Harvard University Press, Cambridge Mass. (1969).

11. In "The Conceptual Development of Quantum Mechanics" Max Jammer points out Bohr's interest in Kierkegaard's philosophy and certain conceptual parallels, in particular between Kierkegaard's qualitative jump and Bohr's irreducible quantum jumps. One can also find strong parallels in the dialectical approach to teaching (e.g. Bohr's famous remark about the complementarity between truth and clarity). The whole viewpoint that Bohr should have been influenced from Kierkegaard is criticized in the following article:

a D. Favrholdt, Danish Yearbook of Philosophy, 13, 206 (1976).
Another paper points out that the philosophical teacher of Bohr in his youth, H. Høffding, although an expert in Kierkegaard's philosophy, was more inspired by the continuity thinking of Leibniz than by Kierkegaard's jump, and that the continuity concept is important in Bohr's correspondence pronciple:
b J. Witt-Hansen, Danish Yearbook of Philosophy, 17, 31 (1980).

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b D. Karnopp and R. C. Rosenberg, "System Dynamics: A Unified Approach", Michigan State University, (1971).

13. H. B. Callen and T. A. Welton, Phys. Rev. 83, 34 (1951).

14. I. R. Senitzky, Phys. Rev. 119, 670 (1960).

15. G. Kron, "Tensor Analysis of Networks", Mc Donald, London (1939,1965).

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17. A. Daneri, A. Loinger and C. M. Prosperi, Nuclear Physics 33, 297 (1962).

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24. E. Santos, Phys. Lett. 101A, 379 (1984).

25. An account of the Furry-hypothesis in connection with Bell's inequalities can be found in:
A. Baracca, D. J. Bohm, B. J. Hiley and A. E. G. Stuart, Nuov. Cim. 28B, 453 (1975).

26. W. Bothe und H. Geiger, Zeitschr. f. Physik, 32, 44 (1925).

27. G. Faraci, D. Gutkowski, S. Notarrigo and A. R. Pennisi, Lett. Nuov. Cim. 9, 607 (1974).

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Vejleder: Anders Madsen.

2/78 "OPTIMERING" - Menneskets forøgede beherskelsesmuligheder af natur og samfund.
Projektrapport af Tom J. Andersen, Tommy R. Andersen, Gert Kreinøe og Peter H. Lassen.
Vejleder: Bernhelm Booss.

3/78 "OPGAVESAMLING", breddekursus i fysik.
Lasse Rasmussen, Aage Bonde Kræmmer, Jens Højgaard Jensen.
Nr. 3 er a jour ført i marts 1984

4/78 "TRE ESSAYS" - om matematikundervisning, matematiklæreruddannelsen og videnskabsrindalismen.
Mogens Niss.
Nr. 4 er p.t. udgået.

5/78 "BIBLIOGRAFISK VEJLEDNING til studiet af DEN MODERNE FYSIKS HISTORIE".
Helge Kragh.
Nr. 5 er p.t. udgået.

6/78 "NOGLE ARTIKLER OG DEBATINDLÆG OM - læreruddannelse og undervisning i fysik, og - de naturvidenskabelige fags situation efter studenteroprøret".
Karin Beyer, Jens Højgaard Jensen og Bent C. Jørgensen.

7/78 "MATEMATIKKENS FORHOLD TIL SAMFUNDSØKONOMIEN".
B.V. Gnedenko.
Nr. 7 er udgået.

8/78 "DYNAMIK OG DIAGRAMMER". Introduktion til energy-bond-graph formalismen.
Peder Voetmann Christiansen.

9/78 "OM PRAKSIS' INDFLYDELSE PÅ MATEMATIKKENS UDVIKLING". - Motiver til Kepler's:"Nova Stereometria Doliorum Vinarioum".
Projektrapport af Lasse Rasmussen.
Vejleder: Anders Madsen.

10/79 "TERMODYNAMIK I GYMNASIET".
Projektrapport af Jan Christensen og Jeanne Mortensen.
Vejledere: Karin Beyer og Peder Voetmann Christiansen.

11/79 "STATISTISKE MATERIALER"
red. Jørgen Larsen

12/79 "LINEÆRE DIFFERENTIALLIGNINGER OG DIFFERENTIALLIGNINGSSYSTEMER".
Mogens Brun Heefelt
Nr. 12 er udgået

13/79 "CAVENDISH'S FORSØG I GYMNASIET".
Projektrapport af Gert Kreinøe.
Vejleder: Albert Chr. Paulsen

14/79 "BOOKS ABOUT MATHEMATICS: History, Philosophy, Education, Models, System Theory, and Works of Reference etc. A Bibliography".
Else Høyrup.

Nr. 14 er p.t. udgået.

15/79 "STRUKTUREL STABILITET OG KATASTROFER i systemer i og udenfor termodynamisk ligevægt".
Specialeopgave af Leif S. Striegler.
Vejleder: Peder Voetmann Christiansen.

16/79 "STATISTIK I KRÆFTFORSKNINGEN".
Projektrapport af Michael Olsen og Jørn Jensen.
Vejleder: Jørgen Larsen.

17/79 "AT SPØRGE OG AT SVARE i fysikundervisningen".
Albert Christian Paulsen.

18/79 "MATHEMATICS AND THE REAL WORLD", Proceedings of an International Workshop, Roskilde University Centre, Denmark, 1978. Preprint.
Bernhelm Booss & Mogens Niss (eds.).

19/79 "GEOMETRI, SKOLE OG VIRKELIGHED".
Projektrapport af Tom J. Andersen, Tommy R. Andersen og Per H.H. Larsen.
Vejleder: Mogens Niss.

20/79 "STATISTISKE MODELLER TIL BESTEMMELSE AF SIKRE DOSER FOR CARCINOGENE STOFFER".
Projektrapport af Michael Olsen og Jørn Jensen.
Vejleder: Jørgen Larsen.

21/79 "KONTROL I GYMNASIET - FORMÅL OG KONSEKVENSER".
Projektrapport af Crilles Bacher, Per S. Jensen, Preben Jensen og Torben Nysteen.

22/79 "SEMIOTIK OG SYSTEMEGENSKABER (1)".
1-port lineært response og støj i fysikken.
Peder Voetmann Christiansen.

23/79 "ON THE HISTORY OF EARLY WAVE MECHANICS - with special emphasis on the role of realitivity".

24/80 a+b "MATEMATIKOPFATTELSER HOS 2.G'ERE".
1. En analyse. 2. Interviewmateriale.
Projektrapport af Jan Christensen og Knud Lindhardt Rasmussen.
Vejleder: Mogens Niss.

Nr. 24 a+b er p.t. udgået.

25/80 "EKSAMENSOPGAVER", Dybdemodulet/fysik 1974-79.

26/80 "OM MATEMATISKE MODELLER".
En projektrapport og to artikler.
Jens Højgaard Jensen m.fl.

27/80 "METHODOLOGY AND PHILOSOPHY OF SCIENCE IN PAUL DIRAC's PHYSICS".
Helge Kragh.

28/80 "DIELEKTRISK RELAXATION - et forslag til en ny model bygget på væskernes viscoelastiske egenskaber".
Projektrapport, speciale i fysik, af Gert Kreinøe.
Vejleder: Niels Boye Olsen.

29/80 "ODIN - undervisningsmateriale til et kursus i differentialligningsmodeller".

Projektrapport af Tommy R. Andersen, Per H.H. Larsen og Peter H. Lassen.

Vejleder: Mogens Brun Heefelt

30/80 "FUSIONSENERGIEN - - - ATOMSAMFUNDETS ENDESTATION".

Oluf Danielsen.

Nr. 30 er udgået.

Udkommer medio 1982 på Fysik-, Matematik- og Kemilærernes forlag.

31/80 "VIDENSKABSTEORETISKE PROBLEMER VED UNDERVISNINGSSYSTEMER BASERET PÅ MÆNGDELÆRE".

Projektrapport af Troels Lange og Jørgen Karrebæk.

Vejleder: Stig Andur Pedersen.

Nr. 31 er p.t. udgået

32/80 "POLYMERE STOFFERS VISCOELASTISKE EGENSKABER - BELYST VED HJÆLP AF MEKANISKE IMPEDANSMÅLINGER OG MÖSSBAUEREFFEKTMÅLINGER".

Projektrapport, speciale i fysik, af Crilles Bacher og Preben Jensen.

Vejledere: Niels Boye Olsen og Peder Voetmann Christiansen.

33/80 "KONSTITUERING AF FAG INDEN FOR TEKNISK-NATURVIDENSKABELIGE UDDANNELSER. I-II".

Arne Jakobsen.

34/80 "ENVIRONMENTAL IMPACT OF WIND ENERGY UTILIZATION".

ENERGY SERIES NO.1.

Bent Sørensen.

Nr. 34 er udgået.

Publ. i "Renewable Sources of Energy and the Environment", Tycooli International Press, Dublin, 1981.

35/80 "HISTORISKE STUDIER I DEN NYERE ATOMFYSIKS UDVIKLING".

Helge Kragh.

36/80 "HVAD ER MENINGEN MED MATEMATIKUNDERVISNINGEN ?".

Fire artikler.

Mogens Niss.

37/80 "RENEWABLE ENERGY AND ENERGY STORAGE".

ENERGY SERIES NO.2.

Bent Sørensen.

38/81 "TIL EN HISTORIETEORI OM NATURERKENDELSE, TEKNOLOGI OG SAMFUND".

Projektrapport af Erik Gade, Hans Hedal, Henrik Lau og Finn Physant.

Vejledere: Stig Andur Pedersen, Helge Kragh og Ib Thiersen.

Nr. 38 er p.t. udgået

39/81 "TIL KRITIKKEN AF VÆKSTØKONOMIEN".

Jens Højgaard Jensen.

40/81 "TELEKOMMUNIKATION I DANMARK - oplæg til en teknologivurdering".

Projektrapport af Arne Jørgensen, Bruno Petersen og Jan Vedde.

Vejleder: Per Nørgaard.

Nr. 40 er p.t. udgået

41/81 "PLANNING AND POLICY CONSIDERATIONS RELATED TO THE INTRODUCTION OF RENEWABLE ENERGY SOURCES INTO ENERGY SUPPLY SYSTEMS".

ENERGY SERIES NO.3.

Bent Sørensen.

42/81 "VIDENSKAB TEORI SAMFUND - En introduktion til materialistiske videnskabsopfattelser".

Helge Kragh og Stig Andur Pedersen.

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2. "ADVANTAGES AND DISADVANTAGES OF DECENTRALIZATION".

ENERGY SERIES NO.4.

Bent Sørensen.

44/81 "HISTORISK UNDERSØGELSE AF DE EKSPERIMENTELLE FORUDSÆTNINGER FOR RUTHERFORDS ATOMMODEL".

Projektrapport af Niels Thor Nielsen.

Vejleder: Bent C. Jørgensen.

45/82

46/82 I+II "EKSEMPLARISK UNDERVISNING OG FYSISK ERKENDELSE - ILLUSTRERET VED TO EKSEMPLER".

Projektrapport af Torben O. Olsen, Lasse Rasmussen og Niels Dreyer Sørensen.

Vejleder: Bent C. Jørgensen.

47/82 "BARSEBÄCK OG DET VÆRST OFFICIELT-TÆNKELIGE UHELD".

ENERGY SERIES NO.5.

Bent Sørensen.

48/82 "EN UNDERSØGELSE AF MATEMATIKUNDERVISNINGEN PÅ ADGANGSKURSUS TIL KØBENHAVNS TEKNIKUM".

Projektrapport af Lis Eilertzen, Jørgen Karrebæk, Troels Lange, Preben Nørregaard, Lissi Pedersen, Laust Rishøj, Lill Røn, Isac Showiki.

Vejleder: Mogens Niss.

49/82 "ANALYSE AF MULTISPEKTRALE SATELLITBILLEDER".

Projektrapport af Preben Nørregaard.

Vejledere: Jørgen Larsen & Rasmus Ole Rasmussen.

50/82 "HERSLEV - MULIGHEDER FOR VEDVARENDE ENERGI I EN LANDSBY". ENERGY SERIES NO.6.

Rapport af Bent Christensen, Bent Hove Jensen, Dennis B. Møller, Bjarne Laursen, Bjarne Lillethorup og Jacob Mørch Pedersen.

Vejleder: Bent Sørensen.

51/82 "HVAD KAN DER GØRES FOR AT AFHJÆLPE PIGERS BLOKERING OVERFOR MATEMATIK?"

Projektrapport af Lis Eilertzen, Lissi Pedersen, Lill Røn og Susanne Stender.

52/82 "DESUSPENSION OF SPLITTING ELLIPTIC SYMBOLS"

Bernhelm Booss & Krzysztof Wojciechowski.

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Arne Jakobsen & Stig Andur Pedersen.

54/82 "FUTURES RESEARCH" - A Philosophical Analysis of Its Subject-Matter and Methods.

Stig Andur Pedersen & Johannes Witt-Hansen.

55/82 "MATEMATISKE MODELLER" - Litteratur på Roskilde Universitetsbibliotek.
En bibliografi.
Else Høyrup.

Vedr. tekst nr. 55/82:
Se også tekst 62/83.

56/82 "ÉN - TO - MANGE" -
En undersøgelse af matematisk økologi.
Projektrapport af Troels Lange.
Vejleder: Anders Madsen.

57/83 "ASPECT EKSPERIMENTET" -
Skjulte variable i kvantemekanikken?
Projektrapport af Tom Juul Andersen.
Vejleder: Peder Voetmann Christiansen.

Nr. 57 er udgået.

58/83 "MATEMATISKE VANDRINGER" - Modelbetragtninger over spredning af dyr mellem småbiotoper i agerlandet.
Projektrapport af Per Hammershøj Jensen & Lene Vagn Rasmussen.
Vejleder: Jørgen Larsen.

59/83 "THE METHODOLOGY OF ENERGY PLANNING".
ENERGY SERIES NO. 7.
Bent Sørensen.

60/83 "MATEMATISK MODEKSPERTISE" - et eksempel.
Projektrapport af Erik O. Gade, Jørgen Karrebæk og Preben Nørregaard.
Vejleder: Anders Madsen.

61/83 "FYSIKS IDEOLOGISKE FUNKTION", som et eksempel på en naturvidenskab - historisk set.
Projektrapport af Annette Post Nielsen.
Vejledere: Jens Høyrup, Jens Højgaard Jensen og Jørgen Vogelius.

62/83 "MATEMATISKE MODELLER" - Litteratur på Roskilde Universitetsbibliotek.
En bibliografi. 2. rev. udgave
Else Høyrup

63/83 "CREATING ENERGY FUTURES: A SHORT GUIDE TO ENERGY PLANNING".
ENERGY SERIES No. 8
David Crossley & Bent Sørensen

64/83 "VON MATHEMATIK UND KRIEG".
Bernhelm Booss og Jens Høyrup

65/83 "ANVENDT MATEMATIK - TEORI ELLER PRAKSIS".
Projektrapport af Per Hedegård Andersen, Kirsten Habekost, Carsten Holst-Jensen, Annelise von Moos, Else Marie Pedersen, Erling Møller Pedersen.
Vejledere: Bernhelm Booss & Klaus Grünbaum

66/83 "MATEMATISKE MODELLER FOR PERIODISK SELEKTION I ESCHERICHIA COLI".
Projektrapport af Hanne Lisbet Andersen, Ole Richard Jensen og Klavs Frisdahl.
Vejledere: Jørgen Larsen og Anders Hede Madsen

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Projektrapport af Lone Biilmann og Lars Boye

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Projektrapport af Lise Odgård Gade, Susanne Hansen, Michael Hviid, Frank Mølgård Olsen.

Vejleder: Jørgen Larsen.

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Albert Chr. Paulsen

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Projektrapport af Hanne Lisbet Andersen, Torben J. Andreasen, Svend Åge Houmann, Helle Glerup Jensen, Keld Fl. Nielsen, Lene Vagn Rasmussen.

Vejleder: Klaus Grünbaum & Anders H. Madsen

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Af Niels Boye Olsen og Bent Sørensen

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Vejleder: Bernhelm Booss

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Mogens Brun Heefelt

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Specialerapport af Jørgen Wind Petersen og Jan Christensen

Vejleder: Niels Boye Olsen

82/84 "MATEMATIK- OG FYSIKUNDERVISNINGEN I DET AUTOMATISEREDE SAMFUND"

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Red.: Jens Højgaard Jensen, Bent C. Jørgensen og Mogens Niss

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Jens Højgaard Jensen, Mogens Niss m. fl.

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af Albert Chr. Paulsen

91/85 "KVANTETEORI FOR GYMNASIET"
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Projektrapport af: Birger Lundgren, Henning Sten Hansen og John Johansson

Vejleder: Torsten Meyer

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Projektrapport af: Birger Lundgren, Henning Sten Hansen og John Johansson

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